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# THE CLASSICAL QUARTERLY

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# THE CLASSICAL QUARTERLY

JULY-OCTOBER, 1932.

## DISTICHA DE MENSIBVS.

(ANTH. LAT. RIES. 665, POET. LAT. MIN. BAEHR. PP. 210 F.)

THE twenty-four lines of this poem have been preserved only by the cod. Sangallensis 878 (variously assigned to saec. IX or IX-XI or XI-XII), whence it was edited in 1863 by K. Schenkl *Sitzungsb. d. phil.-hist. Cl. d. kais. Akad. d. Wissensch.* (Vienna) XLIII p. 71. A single line, the last, exists also in the cod. Bernensis 108 saec. IX. Fifteen survive in a MS of the 17th century now divided into two parts, Barberinus (or Barberinianus) XXXI 39 and Vaticanus 9135, the former containing the hexameters 3, 5, 15, 17, 19, 21, 23 and a spurious hexameter in lieu of 1, the latter the pentameters 2, 4, 14, 16, 18, 20, 22, 24. The eight consecutive lines 6-13 are therefore missing from both; and in this imperfect and interpolated form the poem was edited by Mommsen with Haupt's assistance in *C.I.L.* I p. 411.

It may be read in *C.I.L.* I pp. 332 f. that the calligrapher Furius Dionysius Philocalus executed in A.D. 354 a manuscript calendar on twenty-four pages, two for each month of the year. On the first page of each pair was an emblematic picture of the month with a descriptive tetrastich in the margin; on the opposite page the fasti corresponding. At the foot, hexameter on the one page, pentameter on the other, were the distichs of which I am now to speak.

All the pictures and all the fasti, but with no verses accompanying, are extant in the cod. Vindob. 3146 written about A.D. 1480. A less complete MS was discovered at Arras in the 17th century by the great collector Peiresc, who assigned it to the 9th or 10th. It lacked the pictures of January, April, May, June, and July, and the fasti of March, April, May, and June; but on all the pages which it contained it preserved the verses proper to them. It is now lost again, but two copies of it were made before its disappearance and still survive. These are the cod. Bruxellensis 7542-8 and the MS now divided into Barb. XXXI 39 and Vat. 9135. Except for a spurious page in Barb., of which more anon, they agree with their lost original as described by Peiresc in possessing only seven pictures, Feb. March, Aug. Sept. Oct. Nov. Dec., and only eight tables of fasti, Jan. Feb. Jul. Aug. Sept. Oct. Nov. Dec.: Brux. further omits the distichs, and the five tetrastichs which describe the missing pictures; Barb. and Vat. exhibit on each page the verse or verses belonging to it, the tetrastichs in minuscules, the distichs in capitals.<sup>1</sup>

<sup>1</sup> The seven figures of Feb. March, Aug. Sept. Oct. Nov. Dec. were reproduced from E.ux. by Bucherius in his *commentarius in Victorium Aquitanum* (1633) pp. 275-88, and the whole series of twelve from Vindob. by Lambecius in his *bibl. Caesarea append. comment. l. IV addit. I* (1671)

pp. 271-303. Strzygowski in his *Calenderbilder d. Chronogr. v. Jahre 354, Jahrb. d. kais. deutsch. archaeol. Inst., Ergänzungsheft I* (1888) gives some examples from all three MSS. The fasti are edited from Vindob. and Brux. by Mommsen in *C.I.L.* I pp. 334-56 (ed. 2 pp. 256-78).

Philocalus was a mere executant, and no part of his work, scientific or artistic or literary, originated with him. 'Auctor horum fastorum non fuit Philocalus, sed sunt vulgares eius aetatis publica auctoritate editi' says Mommsen *C.I.L.* I p. 332; Baehrens *poet. Lat. min.* I p. 204 denies with reason that either the distichs or the tetrastichs belong to so late an age, for their general purity and elegance are almost Augustan; and if the tetrastichs are older, so also must be the designs which they describe. But a limit to the antiquity of the distichs is fixed by the *concedō* of line 23. The first dactylic poet to make a palimbacchius out of a molossus by shortening a final *o* is Persius, who has *sartagō* and perhaps *accedō* (P, *accede* A B).

The whole series of tetrastichs, 48 lines, has found its way, whether from Philocalus' calendar or from an independent source, into many MSS, some as early as the 9th century, and is now *anth. Lat. Ries.* 395 (*poet. Lat. min. Baehr.* I pp. 206-9). The distichs, as appears from what has been said, have only the following authority.

B = Bern. 108, saec. IX: line 24.

S = Sangall. 878, saec. IX-XII: lines 1-24.

P = Barb. XXXI 39 + Vat. 9135, saec. XVII: lines 2-5 and 14-24.

January.

1 primus, Iane, tibi sacratur et omnia mensis

2 undique cui semper cuncta uidere licet.

Attentive readers of *C.I.L.* I p. 411 must have been perplexed by a seeming contradiction. Mommsen there printed the first verse as 'Ianus adest bifrons primusque ingreditur annum', and not only did the silence of his apparatus criticus imply that it was in Barb. XXXI 39 but he expressly declared that this MS contains all the hexameters except for April, May, June, and July. But then he added that the hexameters are written under the pictures of the months; and on p. 333 he had told us that Peiresc's MS, of which he says Barb. is a copy, did not contain the picture of January. Light was cast on the puzzle a quarter of a century later by pp. 56 f. of Strzygowski's *Calenderbilder*, and Mommsen in *chron. min.* I p. 48 admitted that the facts are these. Barb. does contain a picture of January, and this hexameter beneath it; but both are spurious. Comparison with the genuine picture in Vindob. 3416 and the genuine hexameter in Sangall. 878 makes it clear that this page of Barb. has no earlier origin than the 17th century copyist. He knew the descriptive tetrastich—for the tetrastichs were accessible in many places—and he made a picture of January to suit it: the hexameter he besought and obtained from the too complaisant Muse.

*et omnia* is corrupt, and Schenkl's *ut* is no emendation; Baehrens's *eponyme* is thoroughly improbable in a poem whose vocabulary is otherwise classical. Buecheler's *nomine* satisfies the sense, but that can be satisfied with less alteration by

primus, Iane, tibi sacratus it ordine mensis.

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Of *-tus* changed to *-tur* I can remember seven examples in Manilius alone, I 844, 874, II 138, 277, 494, IV 652, V 91, and Ribbeck cites six from the capital MSS of Virgil in *prol.* p. 253. Examples of *it* changed to *et* are too plentiful for counting, and the following could be multiplied many times over: Verg. *georg.* III 507, 517, *Aen.* IV 665, V 558, VI 448, VIII 557, XII 283, 452, *moret.* 103, Hor. *carm.* IV 5 7, *epist.* I 7 55, Luc. III 228, VI 372, 828, X 329, Val. Fl. III 3, 277, Sil. II 521, Stat. *Theb.* II 11, *silu.* I 1 43, IV 3 62, *peruig. Ven.* 29: in Verg. *georg.* III 517 and *Aen.* IV 665 the corruption is presented by the majority of the best MSS. Between *ordine* and *omnia* the half-way house may have been *omine*. The phrase is like Enn. *ann.* 424 Vahl. 'post acer hiemps it' and Verg. *buc.* IV 12 'incipient . . . procedere menses'.

## February.

- 3 umbrarum est alter, quo mense putatur honore  
4 peruia terra dato manibus esse uagis.

## March.

- 5 condita Mauortis magno sub nomine Roma  
6 non habet errorem: Romulus auctor erit.

6-13 om. P. *errore* S, corr. Schenkl.

This can only mean that Rome was founded in the month of March and that Romulus will tell you so if you ask him. Then Romulus will lie; for we all know that Rome was founded XI kal. Mai., where *Roma condita* and *natalis urbis* stand in the fasti. Mommsen's *numine*, proposed before the pentameter was known, destroys the only connexion between the distich and the month.

What was here said about March ought not to be in doubt: Macr. *Sat.* I 12 3 'Romanos olim auctore Romulo annum suum decem habuisse mensibus ordinatum, qui annus incipiebat a Martio', Ouid. *fast.* I 27-39 'tempora digereret cum conditor urbis, in anno | constituit menses quinque bis esse suo . . . Martis erat primus mensis', III 97 f. 'Romulus . . . sanguinis auctori tempora prima dedit', Plut. *Numa* 18 3 μετεκίνησε δὲ καὶ τὴν τάξιν τῶν μηνῶν· τὸν γὰρ Μάρτιον πρῶτον ὄντα τρίτον ἔταξε, πρῶτον δὲ τὸν Ἰανουάριον, ὃς ἦν ἐνδέκατος ὑπὸ Ρωμύλου. This tradition reappears at this place in other of the *carmina de mensibus*: anth. Lat. *Ries.* 394 3 'incipie, Mars, anni felicia fata reducti', 639 3 'Martius antiqui primordia protulit anni', 761<sup>a</sup> 'si nouus a Iani sacris numerabitur annus, | Quintilis falso nomine dictus erit. | si facis, ut fuerant, primas a Marte kalendas, | tempora constabunt ordine ducta suo', Auson. 377 (p. 98 Peip.) 5 f. 'Martius et generis Romani praesul et anni | prima dabat Latiis tempora consulibus'. This meaning, that 'anni initium mensis est Martius' (Seru. *georg.* I 43) and that Romulus will be found to be the authority for the doctrine, was probably conveyed in these words:

Mauortis magno sub nomine tempora condi,

first corrupted to *te roma condi* and then transposed and corrected as we see. I have collected many such progressions in error on pp. lxi ff. of vol. I of my Manilius, and from Manilius himself might be added II 495 *in semet uertunt oculos, in semet uertitur oculis, uertitur in semet oculis* and III 353 *'quaerentur medio terrae celata tumore', tempore, 'tempore quaerentur medio celata tumore'*. The year and its months are *tempora* not only in four of the passages cited above but often enough elsewhere, as in Luc. V 6 *'ducentem tempora Ianum'* and Stat. *silu.* IV 1 19 f. *'sic tempora nasci, | sic annos intrare decet'*; *condere* is used as in Verg. *Aen.* VI 792 f. *'aurea condet | saecula qui rursus Latio'*, Stat. *silu.* IV 1 37 *'altera saecula condes'*, Plin. *n.h.* VII 120 *'a condito aeuo.'*

## April.

7 Caesarem ut Veneris mensi, quo floribus arua

8 prompta uirent, auibus quod sonat omne nemus.

Schenkl proposed (for Riese reports him wrongly) *'Caesareae (or Caesaris et) Veneris mensis'*. I can think of no good reason why Venus in connexion with April should be called Caesarea, ancestress of the Iulii; but between April itself and Caesar there was once, though Schenkl does not mention it, a special link. This poem, as I have said, may belong to Nero's time, and Nero made this month his own: Tac. *ann.* XV 74 1 *'(decernitur) ut . . . mensis . . . Aprilis Neronis cognomentum acciperet'*, Suet. *Ner.* 55 *'mensem . . . Aprilem Neroneum appellauit'*. A poet writing in that reign might say *'Caesar inest Veneris mensi'*; but his verse would not have been perpetuated to the time of Philocalus, and a better conjecture is Baehrens's *at sacer est Veneri mensis*. He assumes the stages *sacer ē, sa-ce-rem, ce-sa-rem*; and one of them is found in *anth. Lat.* Ries. 395 (*poet. Lat. min. Baehr.* I p. 206) 1 *'hic Iani mensis sacer est'* (*sacerem* cod. unus). This distich survives only in S, so that it is lawful to postulate minuscule corruptions.

For *prompta* Riese writes *compta*, which is indeed a less change than *picta* but not a natural word: the *thes. ling. Lat.* has nothing nearer than Ennod. *carm.* I 5 48 *'Eridanus claris radiabat comptus harenis'*. For *quod* Schenkl writes *quo*, which may seem obvious but is far from certain. At Manil. II 380 I have cited errors such as *quod fieri* for *confieri* in the Mediceus of Virgil at *Aen.* IV 116, and *'consonat omne nemus'* is a Virgilian hemistich, *Aen.* V 149 and VIII 305.

## May.

9 hos sequitur laicus toto iam corpore Maius

10 Mercurio et Maia quem tribuisse Ioue.

Riese's *laetus* in the hexameter would not easily have been corrupted to *laicus* and is not as appropriate as one could wish to *corpore*; but I have nothing better to propose. In the pentameter Schenkl's *Maiae . . . inuat* is no more convincing, for the change of dative to ablative, small though it is, was not likely to happen with *tribuisse* close by, and the change of *inuat* to *ioue* is not very small. As *errore* in 6 is written for *errorem*, so *ioue* here would naturally



be *iouem*; and the ablative *Maia* suggests that *Mercurio* is a gloss on some such periphrasis as *genito Maia*: for instance

<fama sato> *Maia quem tribuisse Iouem.*

Mercury is '*Maia satus*' in Stat. *Theb.* II 1, and *fama* without *est* has an accusative and infinitive at Val. Fl. VI 137-9 '*fama . . . Bacchum . . . Arabas fudisse*' and in a relative clause, as here, at Stat. *Theb.* I 699 f. '*ubi fama uolentem . . . umeris subiisse molares*'.

This is not more than a possible remedy; but its possibility can be established by parallels. In Ouid. *art.* I 683 f. '*iam dea laudatae dederat mala praemia fermae | colle sub Idaeo uincere digna duas*' the place of *duas* has been taken in most MSS by the marginal interpretation *Venus*. In Sen. *Oed.* 167 f., for the '*flumina seruat durus senio | nauita crudo*' of the best MS, most of the others give *Charon*. In Ouid. *Ib.* 503 '*quique Lycurgiden letauit et arbore natum*' one of the chief MSS gives *Arcadem Ancaenum*, a correct explanation of *Lycurgiden*. In *pan. Mess.* 55 f. '*nec ualuit lotos coeptos auertere cursus, | cessit et Aetnaeae Neptunius incola rupis*' it is only the frag. Cuiac. that preserves this reading: the rest have *Cyclops*, a correct explanation of *Neptunius incola* in the next verse. I said a good deal on this subject in C.R. XVI (1902) pp. 442-6.

June.

- 11 Iunius ipse sibi causam tibi nominis edit  
12 praegrauida attollens fertilitate sata.

July.

- 13 quam bene, Quintilis, mutasti nomen! honori  
14 Caesareo, Iuli, te pia causa dedit.

13 *quam* Riese, *nam* S, *iam* Buecheler.

*honori* Schenkl, *honore* S.

14 *redit* P.

*Caesareo* Riese, *Caesare* qui P, *Caesari* qui (hoc supra scr.) S, *Caesaris* o Baehrens.

*Iuli* Mommseno *praeunte* Buecheler, *Iulio* S P.

August.

- 15 tu quoque, Sextilis, uenerabilis omnibus annis  
16 numinis Augusti nomen in anno uenis.

16 *nomine* Riese, *tum notus (gratus magnus clarus) eris; in ora uenis* Buecheler; *nomina magna geris* Baehrens.

The word *uenis* neither needs to be altered nor demands the alteration of anything else, for '*uenerabilis uenis*' will be '*uenerabilis es*', like Prop. I 10 25 '*irritata uenit*', Ouid. *met.* VI 37, Manil. IV 382, 457, Sen. *H.f.* 45. If Riese's *nomine* is right, the next word was probably a vocative, like 18 *uelate*, and the slightest change would seem to be *magne*. *nomen* would require something like *adepte*.

## September.

17 tempora maturis September uincta racemis

18 uelate, &lt;e&gt; numero nosceris ipse tuo.

17 tempora maturis Haupt, temporis autumni S, temporibus autumnis P.  
uincta S, uincta P.

18 e Haupt, iam S P.

## October.

19 Octobri laetus portat uindemitor uuas,

20 omnis ager, Bacchi munere, uoce sonat.

20 munera Riese, diues ouat Baehrens.

I have shown by punctuation that the verse stands in need of no amendment. The two ablatives are like Cic. *Tusc.* I 115 'Euthynous potitur, fatorum numine, leto'. *munera* is of course possible, and may even be thought better.

## November.

21 frondibus amissis repetunt sua frigora mensem

22 cum iuga Centaurus celsa retorquet eques.

22 retorquet P, torquet S.

eques om. P.

The November of the Romans is φυλλοχόος μὴν or μείς in Hes. ap. Polluc. I 231, Callim. *Hecal.* pap. Rein. VI col. I u. 12, Apoll. Rhod. IV 217 and elsewhere; and it is annually accompanied by the return of seasonable cold. Q. Cicero (Auson. 383, *frag. poet.* Baehr. p. 315, Morel. p. 79) 10 f. 'ecfetos ramos denudat flamma Nepai, | pigra Sagittipotens iaculatur frigora terris': the leaves fall in the first part of the month, while the sun is still in Scorpius; the cold weather begins when he passes into Sagittarius. In Germ. *frag.* IV 18 ff. 'Scorpion ingrediens (Iuppiter) tua, Liber, munera condit | iamque Sagittiferum scandens sua munera reddit | numquam laetae hiemi', where *munera* has come from the preceding verse, I should again write *frigora*.

*Centaurus eques* is ἵπποτα φήρ, but this title, which in Arat. 664 describes the Southern Centaur, must here signify the zodiacal sign. In the table of fasti on Philocalus' opposite page the words *sol Sagittario* (*sagitari* Vindob.) stand against Nov. 19 (Vindob., 17 Brux. and P) with the figure of Sagittarius beneath them. But what is meant by saying that he 'iuga celsa retorquet'?

In *anth. Lat. Ries.* 874<sup>b</sup> (= Drac. *de mens. P.L.M. Vollm.* V p. 236) the distich on December contains the verse 23 'algida bruma niuans onerat iuga celsa (high hills) pruinis'; but that is no more than a curious accident. Mommsen in *C.I.L.* I p. 411 suggested that *iuga* meant *Libra* or Ζυγός, invisible while the sun is in Scorpius but reappearing as a morning constellation when he enters Sagittarius. *Libra* however, which Cicero once calls *Iugum*, is nowhere called *Iuga*, though Manilius I 611 uses the periphrasis *iuga Chelarum*; and the epithet *celsa* would have no peculiar appropriateness



to this or any equatorial sign. *iuga celsa* as the name of a celestial object would most naturally mean *plaustrum septentrionale*; for Virgil in *georg.* III 140 has 'grauibus . . . *iuga* ducere plaustris' and the synecdoche *iuga* for 'currum' in *Aen.* X 594 'rotis saliens *iuga* deseris', and the Great Bear, which Martial VI 25 2 calls 'Parrhasio . . . Vrsa *iugo*' and of which Seneca *Tro.* 439 says 'clarum . . . septem uerterunt stellae *iugum*', is in a special sense *celsa* because near the north pole. Sen. *H.f.* 129 'signum *celsi* glaciale poli' is the same as 6 f. 'Arctos alta parte glacialis poli | sublime sidus': see too Germ. *phaen.* 23 'pars (axis) *celsa* sub horrifero aquilone', 324 f. 'sidera, quae mundi pars *celsior* aethere uoluit | quaeque uident borean', 459 '*celsior* ad borean qui uergit circulus'. Now the hour when men take most notice of the stars is nightfall; and the first constellation to come forth after sunset in northern latitudes is the Great Bear, 'quam septem stellae primum iam sole remoto | producunt nigrae praebentem lumina nocti', Manil. I 620 f. At Rome 1900 years ago on a November evening it made its appearance low upon the northern horizon, ceasing to descend and beginning to rise again: Centaurus *iuga celsa* retorquebat. Arabia in the course of the same month witnessed phenomena corresponding to the difference of latitude: Plin. *n.h.* II 178 'Septentrio . . . in Arabia Nouembri mense prima uigilia occultus secunda se ostendit'.

December.

23 argumenta tibi mensis concedo December

24 quae sis quam uis annum claudere possis.

24 is contained in B, so that here we have three authorities. In all of them the end of the verse is thus corrupt and defective, but touching their variants at the beginning our information is contradictory. S, according to our only informant Schenkl, has *quae quamuis*. The reading of B is given by Mommsen 'über den Chronographen vom J. 354', *Abh. d. phil.-hist. Classe d. k. sächs. Ges. d. Wissensch.* vol. I (1850) p. 570, as *quae sis quam uis* on the faith of Albert Jahn (p. 558); but in *C.I.L.* I pp. 356 and 411 he gives *quae uis quam uis* and says of this fragment (p. 333) 'accurate descripsit mihi G. Rettig': Riese and Baehrens reproduce the latter testimony, Strzygowski pp. 3 and 81 the former. The reading of P is given by Mommsen *C.I.L.* I p. 411 as *quae sis quam uis*, which Riese and Baehrens repeat: Strzygowski p. 3 says that P has not *quae* but *quale*, and repeats this on p. 81 in the statement 'Das beigesetzte Distichon lautet *argumenta tuis festis concedo, December, | quale sis quamuis annum claudere possis*'; but this statement is so false and negligent as regards *tuis festis* (which is Baehrens's conjecture) that no trust can be put in it. Since *quae sis quam uis* was reported by one witness from B and by another from P, I suspect that it is in fact the reading of both.

Haupt in *C.I.L.* I p. 411 proposed with all due diffidence to write

quaeuis, quis annum claudere poscis <ouans>

'i.e. concedo tibi quaeuis argumenta omnemque materiem Saturnaliorum

iocorum, quibus annum hilare claudere cupis'. Baehrens following in the same path altered *tibi mensis* to *tuis festis*, which was adopted by Riese; in the pentameter Baehrens wrote *quae quavis* leaving the end uncorrected, Riese *quis quemuis* and *<iure> potes*.

*tuis festis*, violent and unlikely though it is, is not inexcusable or causeless, for the addition of *mensis* to the vocative *December* is surprising: 'amnis Nilus' and 'mons Taurus' are familiar modes of speech, but not 'amnis Nile' nor 'mons Taure'. The vagueness and obscurity of *argumenta* is a difficulty common to all the readings. In the pentameter it is pretty clear that S has tried to correct the verse and that the unmetrical form of the corruption is the older.

The distich may be amended thus:

argumenta tibi mensis concedo Decemb<ris>,  
qui squamis annum claudere piscis <amas>.

That is 'tibi, Capricorne, concedo ut indices quae mensis Decembris natura sit'. As with Sagittarius in November, the figure of Capricorn and the words 'sol Capricorno' stand here in the fasti of the opposite page. Capricorn, the zodiacal sign in which the sun concludes the year, is a goat with a fish's tail, and accordingly in *catal. cod. astrol. Gr.* VII p. 208 it has the epithet *λεπιδωτόν*; *argumenta* is used as in *Aetn.* 143 'argumenta dabunt ignoti uera profundum'; and this poet, in saying that Capricorn's scaly tail is an apt symbol of December, has in mind what Isidore tells us in *orig.* III 71 31, 'Capricorni . . . posteriorem partem corporis in effigiem piscis ideo formauerunt ut pluuias eiusdem temporis designarent, quas solet idem mensis plerumque in extremis habere'.

At Nemes. *cyn.* 30 *qui squamosi* appears in the best MS as *quis quam osi*, and I suppose that here there was a like false division, *quis quam is*. If *uis* is rightly reported from B, its *ui* may be a relic of *qui*. It would be a great mistake to defend *quae* by adducing *anth. Lat. Ries.* 622 5 *pelagi capella* and 626 5 *Neptunia capra* (both missing in the *thes. ling. Lat.*), where the feminine gender is a concession to metre.

A. E. HOUSMAN.

TRINITY COLLEGE, CAMBRIDGE.

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## THE *ANTIQUA LEGIO* OF VEGETIUS.

IN the second book of his *Epitoma rei militaris* Vegetius sets himself the task of describing the organization of the *antiqua legio* of the Roman Army, the units into which it was divided, its officers, the arms of its soldiers, and its tactical employment on the field of battle. Interspersed in this account are frequent references to changes that had been subsequently effected and were in operation in the author's lifetime. But although these annotations destroy the synthesis of the book, and the transition from past to present is not always clearly indicated, the disentanglement of Vegetius' own additions from the material provided by his source or sources is not the chief difficulty that confronts the modern historian. For with few exceptions in chapters 4-14 Vegetius describes the *antiqua legio* in the imperfect, the legion of his own time in the present indicative, with the not infrequent addition of the word '*nunc*' or '*hodie*.' The great problem is to discover whether in his account of the *antiqua legio* the author is referring to any one period of Roman History, and, if so, whether its limits can be exactly defined. Many solutions have been attempted. Some scholars have thought that the period is earlier than Hadrian<sup>1</sup>; the more generally accepted theory<sup>2</sup> is that the *antiqua legio* can be identified with the legion as it was constituted in the reign of Diocletian. Recently two German historians have subjected the evidence to fresh consideration, and, while one of them<sup>3</sup> agrees with the majority in selecting the age of Diocletian for his answer to the problem, the other<sup>4</sup> believes that he can date the period to which Vegetius is referring to the late second century A.D.

It is the purpose of this article to examine the arguments upon which these two theories are based, and, when reasons have been found for rejecting their main conclusions, to enquire further whether there is any other period of Roman History with which the *antiqua legio* can be more satisfactorily equated.

The basis of Schenk's theory is an analysis of the probable sources used by Vegetius. In two passages the Epitomator cites the authorities which he has consulted. In Book I 8 he refers to Cato, Celsus, Frontinus and Paternus, and the *Constitutiones* of Augustus, Trajan and Hadrian; in Book II 3 to Cato, Frontinus and *alii complures*. Arguing that Vegetius could have had no interest in concealing the names of his authorities, and that they correspond

<sup>1</sup> Lange, *Historia mutationum rei militaris Romanorum* (1846). Förster, *de fide Flavii Vegetii Renati* (Diss. Bonn., 1879).

<sup>2</sup> Marquardt, *Römische Staatsverwaltung: das Militärwesen*. Grosse, *Römische Militärgeschichte*.

<sup>3</sup> Nischer in Kromayer and Veith's *Kriegsführung und Heerwesen der Griechen und Römer*, pp. 493-4.

<sup>4</sup> Schenk, *Flavius Vegetius Renatus; die Quellen der Epit. rei militaris* (Klio, 1930).



with the military sources mentioned by Lydus<sup>1</sup>, Schenk maintains that the account of the *antiqua legio* must have been derived from one or more of the cited authorities. Therefore a *terminus ad quem* for the period to which the *antiqua legio* belongs is the death of the most recent of the authors cited. Schenk next considers whether the source used by Vegetius for Book II can be more exactly defined. The *antiqua legio* with its ten cohorts of five centuries each will not correspond with the Julio-Claudian legion with its ten cohorts of six centuries apiece. Again it is different from the legion described by Hyginus<sup>2</sup> not only in the number of its centuries, but in two other important ways. In Hyginus the manipular organization is still in force, in Vegetius it has disappeared; in Hyginus there is no legionary cavalry, in Vegetius their strength is 726. Accepting the theory of Von Domaszewski that Hyginus' account of the legion has reference to the period between Vespasian and Trajan (when incidentally there is no epigraphical evidence for the existence of legionary cavalry), Schenk concludes that the *antiqua legio* must be post-Trajanic, and, if so, the source from which Vegetius derived his account can only be Paternus, who is, *ex hypothesi*, included among the *alii complures*. Therefore the *antiqua legio* belongs to the period between the death of Trajan and the death of Paternus (183 A.D.)<sup>3</sup>. To support this contention Schenk ascribes to Hadrian radical changes in the organization of the legion. These he thinks were established by imperial *constitutiones*, and the content of the latter was embodied by Paternus in his treatise, from which Vegetius derived his account of the *antiqua legio*.

Having enunciated his hypothesis that the *antiqua legio* belongs to the second half of the second century A.D., Schenk proceeds to defend it by four main arguments:

1. Hadrian abolished the manipular organization in the legions.
2. In consequence of this reform Hadrian divided each of the ten cohorts into five centuries, and gave to each century a standard of its own.
3. Hadrian reintroduced cavalry into the legions, and increased their number to 726.
4. The legion continued to be commanded by a *legatus*, and the passage in Vegetius referring to the *praefectus legionis*, if properly interpreted, confirms this fact.

Let us consider the validity of these contentions:

1. From two passages in Vegetius<sup>4</sup> it is clear that *manipulus* has no longer the meaning which it had in the Republic and early Principate of a 'company of two centuries,' but is now a synonym for *contubernium*, or a squad of ten men commanded by an officer called *decanus*. In Ammianus Marcellinus *manipulus* has the same connotation<sup>5</sup>. But what evidence is there that this change dates back to Hadrian? Schenk produces in support of his argument two passages

<sup>1</sup> Lyd. *de mag.* I 47, who cites in addition after the name of Paternus Catiline 'not the conspirator' and Vegetius.

<sup>2</sup> *Liber de munitionibus castrorum*, chs. 1-5.

<sup>3</sup> Cf. Dio. LXXII 5 with S.H.A. *vita Comm.* 4, 8; Schenk, *op. cit.*, p. 23.

<sup>4</sup> II 8 and II 13.

<sup>5</sup> E.g. XVII 13, 25; XXI 13, 9; XXIX 5, 39.

from the *Historia Augusta*<sup>1</sup> and Arrian's use of the word *σπεῖρα*<sup>2</sup>. But can any certain conclusions be drawn from them? The language of the authors of the *Historia Augusta* is notoriously inexact, and the phrases 'inter manipula vitam militarem magistrans' and 'decem commanipulones securi percuti iussit' much more probably reflect the vulgar parlance of their own age than the technical military phraseology of the second century A.D. Secondly, although *σπεῖρα* in Arrian means cohort, while by Polybius it is normally used to translate *manipulus*, that does not warrant the conclusion that in Arrian's day the manipulus was defunct. *σπεῖρα* is used by Greek writers on Roman military history as the equivalent of a unit of a few hundred men. Its precise connotation is determined by the period to which they are referring. Thus *σπεῖρα* in Polybius connotes *manipulus* because in the second century B.C. the manipulus was the only tactical and administrative unit inside the legion between the century and the legion itself. When the cohort superseded the manipulus for tactical purposes, it was natural in describing a battle to employ *σπεῖρα* as its Greek equivalent. Now Arrian in his *ἑκταξίς κατ' Ἀλανῶν* is detailing his order of march and of battle. He is dealing in consequence with tactical and not administrative units. It cannot, therefore, be concluded that, because he only refers to cohorts (*σπεῖραι*), the manipular organization, whose function was mainly administrative, had ceased to exist. The abolition by Hadrian of the manipulus in the old sense of the term cannot then be deduced with any certainty from the passages which we have considered; nor indeed have we any evidence that Hadrian changed the internal organization of the legion. That he was keenly interested in the efficiency and discipline of the Roman Army is indicated both by Dio<sup>3</sup> and the extant addresses of the *princeps* himself to the army in Africa<sup>4</sup>. But improvements in armour and tactical manoeuvring and the enforcement of a stricter discipline are goals that can be reached without changes in organization; and if Hadrian did make such drastic alterations as the abandonment of the manipulus and the division of the cohort into five instead of six centuries, it is surely remarkable that no ancient authority has recorded it.

2. In Vegetius' account of the legion the first cohort differs from the remaining nine by being *milliaria*<sup>5</sup>. This organization has been thought by some to be as old as the last century of the Republic<sup>6</sup>, by others to date back to the early Principate<sup>7</sup>. Its existence is definitely attested by Hyginus—'cohors prima duplum habet numerum'<sup>8</sup>. It is further probable that the division of the first cohort into five centuries has the same origin. Its centuries have the titles *primus pilus*, *primus pilus*, *primus princeps prior*, *primus hastatus prior*, *primus princeps posterior*, *primus hastatus posterior*, and it is practically certain that the senior of the two *primipili*, who in inscriptions is called

<sup>1</sup> *vita Hadr.* 10, 2; *vita Pescenn. Nig.* 10, 5.

<sup>2</sup> *ἑκταξ. κατ' Ἀλ.* §§ 1, 5, and 13.

<sup>3</sup> Dio. LXIX 9.

<sup>4</sup> *Dess. I.L.S.* 2487, 9133-4.

<sup>5</sup> II 12.

<sup>6</sup> Schenk, *op. cit.*, p. 20; Domaszewski, *Die Rangordnung*, p. 91.

<sup>7</sup> Parker, *The Roman Legions*, p. 199.

<sup>8</sup> *Op. cit.*, ch. 3.

*primus pilus iterum*, was not in command of a century but served on the staff of the *legatus*<sup>1</sup>. There were in consequence only five centuries in the first cohort. But what of the remaining cohorts? If they were divided by Hadrian into five instead of six centuries on the model of the first cohort, we should expect this to be reflected in inscriptions of the second and third centuries A.D. This is not the case. A dedication put up at Lambaesis to Marcus Aurelius by the '*primi ordines et centuriones et evocatus*' of III *Augusta*<sup>2</sup> gives the names of six centurions in cohorts II, III, IV, V, VII and X, seven centurions in cohorts I and VIII, eight in cohort VI, and five in cohort IX. It is clear that the normal number of centurions to a cohort is six, and the exceptions admit of explanation. The mention of seven centurions in cohorts I and VIII and eight in cohort VI may imply, as Mommsen has suggested, that while the monument was being erected there had been some changes in personnel, and that *all* the centurions whose names are recorded had contributed to the cost of its erection, or, as I prefer to hold<sup>3</sup>, that there were some supernumerary centurions on the strength of the three cohorts who had been seconded for duty away from their unit. The mention of only five centurions in cohort IX probably means no more than that there was an unfilled vacancy. Again two further dedications<sup>4</sup> from Lambaesis, dated to the year A.D. 253<sup>5</sup>, when III *Augusta* was reconstituted, indicate the same organization. The one inscription is slightly later in date than the other; for five of the *optiones* numbered 6-11 in *Dess.* 2445 have been promoted in *Dess.* 2446 to be the *optiones* serving under the five centurions of the first cohort. From this comparison and from the total number of names given in the earlier of the two inscriptions it follows that, while the first cohort had as before five centuries, the remainder are still organized as they had been in the early Principate. Epigraphy therefore proves that so far from any changes in the subdivisions of the cohort having been put into effect by Hadrian, its organization was still unaltered at the beginning of the joint reign of Valerian and Gallienus.

3. The existence of legionary cavalry in the Principate of Hadrian is confirmed both by Arrian in his *ἑκταξίς κατ' Ἀλανῶν*<sup>6</sup> and by the *princeps*' speech to the army in Africa<sup>7</sup>. Now Josephus tells us that in his time the number of cavalry in a legion was 120<sup>8</sup>. This force seems for administrative purposes to have been distributed among the centuries of the legion<sup>9</sup> and for tactical manoeuvring to have been organized in *turmae*, perhaps under the command of the *tribunus semestris*<sup>10</sup>. What then was the strength of the legionary cavalry in the Principate of Hadrian? Schenk holds that it was increased to the figure of 726 given by Vegetius<sup>11</sup>, and he believes that this theory can be confirmed from Arrian. But there are two serious objections to the acceptance of this opinion:

<sup>1</sup> E.g. *Dess. I.L.S.* 2686, 2687, both belonging to the early Principate.

<sup>2</sup> *Ibid.* 2452.

<sup>3</sup> *Op. cit.*, p. 204.

<sup>4</sup> *Dess. I.L.S.* 2445, 2446.

<sup>5</sup> Cf. *ibid.* 531.

<sup>6</sup> *Op. cit.*, § 5.

<sup>7</sup> *Dess. I.L.S.* 2487.

<sup>8</sup> *Bell. Jud.* III. 6, 2.

<sup>9</sup> *Dess. I.L.S.* 2325, 2326.

<sup>10</sup> Domaszewski, *Die Rangordnung*, p. 47.

<sup>11</sup> II 6: the total is given as 730, but the details add up to 726.



(1) If οἱ ἀπὸ τῆς φάλαγγος ἱππεῖς numbered 726, it is surely incredible, as Plew has pointed out<sup>1</sup>, that Arrian should not mention their commander. Schenk's retort that it is implied that they were commanded by their own *decuriones* under the supreme control of the legionary commander, and that similarly Josephus mentions no cavalry commander, is unconvincing. Arrian, it is to be noted, gives us the names of the commanders of the auxiliary *alae*, and, if the legionary cavalry numbered half as many again as an *ala quingenaria*, it is almost impossible to believe that in his detailed instructions for the order of march and battle Arrian should not have assigned particular duties to their commander. On the other hand, if the legionary cavalry numbered no more than 120, their function was probably to act as a bodyguard to the legionary commander, and consequently they would be under his immediate supervision.

(2) Schenk believes that Vegetius' figures are confirmed by the following passage from Arrian's *τέχνη τακτική*<sup>2</sup>: 'ὀνομαστί ἀνακαλεῖσθαι κελεύουσι πάντας ἐφεξῆς τοὺς ἱππέας, δεκαδάρχην πρῶτον καὶ διμοιρίτην ἐπὶ τούτῳ καὶ ὅστις ἐν ἡμιολίῳ μισθοφορᾷ, ἔπειτα τοὺς ἐφεξῆς τῆς δεκαδαρχίας.' This statement of Arrian shows that in his time a *turma* consisted of thirty men with three officers called *decurio*, *duplicarius* and *sesquiplicarius*, and corresponds, it is true, with Vegetius' number for his cavalry *turma*<sup>3</sup>, two of which he assigns to each of the cohorts II-X inclusive, and four to the first cohort, which is *milliaria*. But before this coincidence can be used to support Schenk's theory it must be proved that Arrian is describing a *turma* of legionary cavalry. Now the expressed purpose of the author of *τέχνη τακτική* is to give an historical sketch of 'τὰ ἱππικὰ γυμνάσια ὅσα Ῥωμαῖοι ἱππῆς γυμνάζονται'<sup>4</sup>. He then proceeds to give a description of a number of complicated manoeuvres, which, as their titles show, the Romans learnt from their enemies. There can surely be no doubt that it is the auxiliary *alae* that provide the author with his material. Even if the legionary cavalry were so greatly increased in numbers by Hadrian, as the argument suggests, could they in a few years have mastered the difficult manoeuvres which Arrian describes? Further, if the *turma* of legionary cavalry from the time of Hadrian onwards was commanded by a *decurio* with two subordinate officers called *duplicarius* and *sesquiplicarius*, we should expect them to figure in epigraphy. Inscriptions referring to legionary cavalry are comparatively rare (doubtless because of its lack of importance), but a dedication to Alexander Severus by the *equites* of III *Augusta* at Lambaesis<sup>5</sup> shows that their senior officer was an *optio equitum*. The legionary cavalry in the first half of the third century A.D. were thus numerically insignificant; the increase to 726 had not yet been effected.

4. Vegetius' account of the senior officers is as follows: 'Sed legati imperatoris ex consulibus ad exercitus mittebantur, quibus legiones et auxilia universa obtemperabant in ordinatione pacis uel necessitate bellorum. . . . Proprius autem iudex erat praefectus legionis, qui absente legato tamquam

<sup>1</sup> *Quellenuntersuchungen zur Gesch. des Kaisers Hadrian*, p. 68.

<sup>2</sup> 42, 1.  
<sup>4</sup> 32, 3.

<sup>3</sup> II 14.  
<sup>5</sup> *C.I.L.* VIII 2562.

uicarius ipsius potestatem maximam retinebat.<sup>1</sup> Now until the reign of Gallienus the only legions that were commanded by equestrian *praefecti* were those stationed in Egypt and the three legions called *Parthica*, which were raised by Septimius Severus. All other legions were commanded by senatorial *legati*, who in a province where there was more than one legion were subordinate to the provincial governor, a man of consular rank called *legatus Augusti pro praetore*. In the reign of Gallienus the system of equestrian legionary commanders seems to have been extended all over the Empire. This change is attested by Victor, who says that Gallienus 'ne imperium ad optimos nobilium transferretur, senatum militia uetuit et adire exercitum'<sup>2</sup>, and is confirmed by a series of inscriptions, which describe the legionary commander as *praefectus legionis a(gens) u(ices) l(egati)*.<sup>3</sup> At the same time to make this reform effective Gallienus appears to have separated the civil and the military administration in the provinces. The legionary commanders thus became independent of the civil governors, who came gradually to be drawn exclusively from the equestrian order, and were responsible instead to the district military commanders, who were at first called *praepositi* and later *duces*, and were in either case of equestrian rank<sup>4</sup>. Now the most natural interpretation of the passage quoted from Vegetius is that (a) the legionary commander was called *praefectus legionis*; and (b) when the provincial governor was absent (*absente legato*) his duties fell to the *praefectus legionis*. These two statements are in part incompatible. For the former must refer to the post-Gallienic period, while the latter reflects the practice of the earlier Principate. But this inconsistency is not altogether unintelligible, and two possible explanations of it may be suggested: (1) As late as the reign of Diocletian some of the provincial governors were still of senatorial rank and called *legati pro praetore*<sup>5</sup>, and it may have been wrongly assumed that they retained their former military powers. (2) The title of the legionary commander in the reign of Gallienus was at first *praefectus legionis agens uices legati*, to distinguish him from the inferior *praefectus (castrorum) legionis*. This nomenclature may have been misunderstood and have resulted in the false elucidation '*absente legato tanquam uicarius*.' On the whole then Vegetius' legionary commander may be taken as corresponding with the post-Gallienic *praefectus*.

Schenk's solution is different. He maintains that Vegetius' meaning is that the commander of the legion was the *legatus*, and that in his absence the command fell to the next officer in order of seniority, the *tribunus laticlauius*, who in these circumstances was called *praefectus legionis*. This interpretation is in my judgment inadmissible. In the first place the words '*proprius iudex erat praefectus legionis*' can surely only refer to what was normal, and not to a condition of affairs occasioned by some such accident as sickness or death.

<sup>1</sup> II 9.

<sup>2</sup> *de Caesaribus* 33, §§ 33-34.

<sup>3</sup> *C.I.L.* 3424, 3426, 3469, 4289.

<sup>4</sup> C. W. Keyes, *The Rise of the Equites in the*

*Third Century of the Roman Empire* (Diss., Princeton), p. 54; N. H. Baynes in *J.R.S.* XV (2) 1925, pp. 195-201.

<sup>5</sup> *C.I.L.* II 4102, 4103; Keyes, *op. cit.*, p. 49.

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<sup>1</sup> E.g.

<sup>2</sup> *C.I.L.*

<sup>3</sup> *Id. X*

<sup>4</sup> *Id. X*

Secondly, although it is true that the *tribunus laticlavius* did function for the *legatus legionis* in his absence<sup>1</sup>, he is never while serving in that capacity called *praefectus legionis*, for the very good reason that *praefectus legionis* implies equestrian and not senatorial rank. In inscriptions we find the following titles: *tribunus militum pro legato*<sup>2</sup>; *tribunus militum leg. IV Scyth. uice legati Augusti*<sup>3</sup>; *tribunus militum leg. XXX Ulp. uic. et I Min. Gordianarum in quo honore uices legati sustinuit*<sup>4</sup>. Vegetius' account of the command of the legion thus cannot refer to the second century A.D.; it is only compatible with the conditions introduced by Gallienus.

An examination of the main arguments used by Schenk to support his hypothesis that Vegetius derived his account of the *antiqua legio* from Paternus has shown that none of them is satisfactory. Neither the new organization of the cohorts, nor the increased strength of the cavalry, nor the title of the legionary commander can be made to correspond with the constitution of the legion as we know it from literary and epigraphical evidence of the second century A.D. We must therefore conclude that chapters 4-14, and probably 21, 22 and 25, were derived from a later source than Paternus, which Vegetius has included without naming it among the *alii complures* whom he consulted.

On the other hand, Schenk's ascription of chapters 19 and 20 to Paternus, resting as it does upon a comparison of the language of Vegetius, chapter 19, with that of Paternus as recorded in a section of the *Digest*<sup>5</sup>, is probably right, while chapters 15-17, which describe the *acies* in terms of *principes*, *hastati* and *triarii*, may refer, as he suggests, to the pre-Marian epoch<sup>6</sup>. The accounts, therefore, of organization, administration and tactical dispositions seem to have been derived from three different sources, and, even though the description of the *acies* (if allowance is made for the survival in it of obsolete terms)<sup>7</sup> and of the administrative details may be taken as consonant with the history of the army in the early Principate, the organization of the *antiqua legio* cannot belong to such an early date. Before offering a solution of this last problem, I propose to consider the theory put forward by Nischer in Kromayer and Veith's *Kriegsführung und Heerwesen*.

In his main conclusions Nischer is in agreement with Grosse, but differs from him in his treatment of Vegetius' account of the strength and organization of the cohort. The value of this detailed criticism and reconstruction I shall consider in an appendix; for the present I shall confine myself to the proposed chronological solution.

The period to which the *antiqua legio* belongs is according to this theory the reign of Diocletian. This opinion is supported by three arguments: (1) The statement of Vegetius that the commander of the legion is the *praefectus legionis* shows that the period is post-Gallienic. (2) Gallienus was the founder

<sup>1</sup> E.g. Tac. *Ann.* XV 28; *id. Hist.* III 9.

<sup>2</sup> *C.I.L.* X 4749.

<sup>3</sup> *Id.* XI 1834.

<sup>4</sup> *Id.* XIII 6763.

<sup>5</sup> *Digest* XLIX 16, 2; Schenk, *op. cit.*, p. 35.

<sup>6</sup> *Op. cit.*, p. 35.

<sup>7</sup> Lammert, *Die Römische Taktik zu Beginn der Kaiserzeit*, *Philologus Suppl.* XXIII (1931), p. 30.



of mobile cavalry contingents organized and operating separately from the infantry<sup>1</sup>. Among these contingents are units called *promoti*; these were formed out of the old legionary cavalry, and so the legion lost its cavalry, which were not restored to it till the reign of Diocletian. (3) Diocletian raised legions organized on the old lines, that is, with an effective strength of 5,500 infantry each, and a complement of legionary cavalry estimated at 660. From these premises it follows that the *antiqua legio* of Vegetius must be later than Gallienus, but cannot be earlier than Diocletian. Let us examine these contentions.

With the proposed *terminus a quo* we need not further concern ourselves; our previous discussion has shown its validity. Nor is there any reason to question the statement that Gallienus was the founder of independent cavalry contingents, even if, as we shall see, the origin of the *promoti* may be somewhat later in date. But there are three questions which must be raised: (1) Is it true that the *promoti* had no connection with the legions from which they were drawn? (2) Upon what evidence is the statement based that Diocletian disbanded these mobile units, and restored to the legion its cavalry? (3) Were Diocletian's new legions of the strength that is suggested?

1. An answer to the first question may be obtained by considering the meaning of the word *uexillatio*. In the first two centuries of the principate *uexillatio* is used to designate a detachment of infantry operating away from its main body, but still on the strength of its parent-legion. On the other hand, when Constantine separated the legionary cavalry permanently from the infantry, *legio* is the term used for infantry, and *uexillatio* the term for cavalry<sup>2</sup>. This implies that the now independent cavalry units were in origin detachments from the legion when it was composed both of infantry and cavalry. When did this change in the meaning of the word *uexillatio* occur? In the Grenoble Inscription of 269 A.D.<sup>3</sup> *uexillationes* is still the term for infantry detachments, and the cavalry units are called simply *equites*. But by the beginning of Diocletian's reign it appears from two rescripts, one of which is earlier than 293 A.D., that *uexillatio* has acquired its new meaning<sup>4</sup>. That these *uexillationes*, however, were not yet regarded as altogether independent of the legions from which they were drawn is attested by a *papyrus* of 302 A.D., of which the relevant words are, 'στρατιώτης ἰππεὺς προμωτῶν σεκούντων ἀπὸ λεγεῶνος β' Τραιάνης'<sup>5</sup>. Now although Gallienus is the reputed founder of mobile cavalry contingents recruited from Illyricum, the existence of *promoti* cannot be inferred till the time of Aurelian<sup>6</sup>, and is first definitely attested in a *papyrus* of 293 A.D.<sup>7</sup>. The

<sup>1</sup> Zosimus I 52, 3; Ritterling, *Festschrift f. O. Hirschfeld*, p. 347.

<sup>2</sup> *Notitia Dignitatum* Or. V 27, 33; Occ. VI.53.

<sup>3</sup> *Dess. I.L.S.* 569.

<sup>4</sup> *Cod. Just.* X 55, 3, 'ueteranis ita demum onerum et munerum personalium uacatio iure conceditur, si post uicesimum annum militiae quam in legione uel uexillatione honestam uel causariam missionem consecuti esse ostendantur'; cf. *ibid.* VII 64, 9.

<sup>5</sup> Grenfell-Hunt *Gk. Pap.* II 74; cf. Stein, *Gesch. d. spätöm. Reiches*, p. 92, n. 1.

<sup>6</sup> Zosimus I 52, 3; cf. *Not. Dign. Or.* XXXII-XXXVII, where *equites promoti Illyriciani* are mentioned in conjunction with *equites Dalmatae Illyriciani* and *Mauri Illyriciani*. The inference is that in origin all these units date back to Aurelian.

<sup>7</sup> Grenfell-Hunt *Gk. Pap.* IV 110.

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<sup>1</sup> *Ox.*

<sup>2</sup> *Not.*



probability then is that Gallienus, with his realization of the importance of cavalry for Eastern campaigns, not only raised independent units from Illyricum, but also increased the number of legionary cavalry from 120 to 726. The latter were subsequently used, like the old infantry detachments, as *uexillationes* separate from their legion, but were still in the time of Diocletian regarded as having a definite connection with the legion from which they were drawn. It follows that the creation of the *promoti* did not imply the extinction of the legionary cavalry, but merely their employment as *uexillationes* in the same way as detachments of infantry had previously been temporarily separated from their main body.

2. Our answer to the first question has shown that Diocletian did not disband the *promoti*. In addition to the two *papyri* already cited, we have further references to mobile cavalry units in a third *papyrus*<sup>1</sup>, which records that Diocletian's expeditionary force in Egypt comprised, in addition to infantry detachments under the command of nine *praepositi*, *comites* and an unnamed *uexillatio* of cavalry. Now in the later Palatine troops *promoti* and *comites* figure as the most distinguished *uexillationes*<sup>2</sup>. The probability then is (a) that the separation of cavalry from infantry began with Gallienus or Aurelian, continued under their successors, including Diocletian, and reached its final consummation with Constantine; and (b) that the cavalry contingents drawn from the legions at first retained, on the model of the old *uexillationes* of infantry, a connection with the units from which they were drawn, and finally were constituted by Constantine in complete independence of the infantry.

To maintain their contention, however, that Diocletian restored to the legion its cavalry, Grosse and Nischer cite the evidence of the following inscription from Rome which has reference to a body of troops called *lanciarii*: 'd.m.s. Val. Maxentio aeq. ex numero lanciarorum (sic) uixit an. XXVI, mil. an. VI ischola aequitum b.m.f.<sup>3</sup>' But is it legitimate to draw any inferences from this inscription about the existence or non-existence of cavalry in frontier-legions? The identification of the *lanciarii* is a matter of doubt. Nischer maintains<sup>4</sup> that they were in origin part of the praetorian guard, and had no connection with the later Palatine legions of that name. If so, then clearly the inscription cited above can have no possible bearing on the history of the Diocletianic legions. But even if *lanciarii* were a mobile legionary unit, which formed part of the *sacer comitatus* of Diocletian<sup>5</sup>, we are not entitled from such evidence as we have of their organization to argue about the constitution of the frontier-legions raised by Diocletian. Further, although Mommsen has dated the inscription which we are considering to the age of Diocletian, the mention of a *schola equitum* suggests rather the reign of Constantine, and it may well be that the *lanciarii*, to whom this *schola equitum* seems to have been

<sup>1</sup> *Ox. Pap.* I 89, n. 43.

<sup>2</sup> *Not. Dign. Or.* V 28, 29, 30; *Occ.* VI 43, 44.

<sup>3</sup> *Dess. I.L.S.* 2791.

<sup>4</sup> *Op. cit.*, p. 492.

<sup>5</sup> *Cf. Dess. I.L.S.* 2781.

attached, were the *Palatini* of that name,<sup>1</sup> whether they were related or unrelated to the Diocletianic *lanciarii*.

3. In his paper on 'The Army Reforms of Diocletian and Constantine'<sup>2</sup> Nischer describes the system of Diocletian as 'a preference for the first and last time for large units,' and assigns to his legions an effective strength of 5,500. It is difficult to accept this opinion. Perhaps the legions that were raised earliest in Diocletian's reign conformed in size and strength with those already in existence if, in addition to their garrison duties, they were required to furnish troops for foreign campaigns<sup>3</sup>. But the units raised later in his reign were probably much smaller, and perhaps did not exceed a strength of 1,000. When Egypt was made tripartite, the province of *Herculia* was garrisoned by detachments from the two Dacian legions which had, it would seem, formed part of Diocletian's expeditionary force<sup>4</sup>. If then units of this size were deemed adequate for one province, it is not improbable that the same principle was followed in other provinces where new Diocletianic legions are found. This hypothesis may be applied with some degree of certainty to the legions stationed by Diocletian on the Euphrates frontier which were not called upon to provide contingents for the mobile field army. The policy of Diocletian was not determined by stereotyped principles but by the military requirements of the Empire. If then a possible exception is made of the units raised earliest in his reign, the Diocletianic legion is not the historical counterpart of Vegetius' *antiqua legio*.

The points which have emerged from an examination of the theories of Schenk and Nischer may now be summarized:

1. The *antiqua legio*, 6,100 strong and organized in ten cohorts of five centuries apiece, is not older than the reign of Gallienus nor later than the early years of Diocletian. For the dedications made by the *optiones* of III *Augusta* to Valerian and Gallienus show that with the exception of the first cohort the remainder were still divided into six centuries, while there is reason to think that the legions raised by Diocletian, at least after 297 A.D., were of a much smaller effective strength than those which were already in existence.

2. The cavalry contingents called *promoti*, whose existence can be inferred in the reign of Aurelian, imply an increase in the number of legionary cavalry, which was most probably carried out by Gallienus. These *promoti* are best regarded as *uexillationes* from the legions, and, although operating independently from the infantry, still retained as late as 302 A.D. a connection with the units from which they were drawn.

3. The legionary commander is the equestrian *praefectus legionis*, and the government of the imperial provinces according to Vegetius is still in the

<sup>1</sup> *Not. Dign. Or.* V 42, VI 47.

<sup>2</sup> *J.R.S.* XIII, 1923, p. 9.

<sup>3</sup> Perhaps the Joviani and Herculiani of the later Palatine Guard were in origin detachments from I Jovia and II Herculia. More probably they were formed out of auxiliary cohorts. (*Not.*

*Dign. Or.* V 43, 44; *Occ.* V 145, 146=VII 3, 4; Vegetius I 17; Jullian, 'La carrière d'un soldat au quatrième siècle' in *Bull. Epig.* IV [1884], pp. 1-12.)

<sup>4</sup> Eutropius IX 23; *Latev. Veron.* (Seeck, *Not. Dign.*, p. 247); *Ox. Pap.* I 89, n. 43.

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hands of senatorial *legati pro praetore*. There are no examples of senatorial *legati legionum* after the reign of Gallienus; but although the substitution of equestrian for senatorial provincial governors had not been universally carried out till the time of Diocletian, such *legati pro praetore* as continued to function had lost their military authority.

The answer to the problem becomes apparent. The only period to which the legion as described by Vegetius can belong is that which falls between the reigns of Gallienus and Diocletian, and which may be approximately dated to the years 260-290 A.D.<sup>1</sup> This solution admits of a satisfactory explanation of Vegetius' account of the legionary cavalry and the legionary commander, and, even if there is no evidence that Gallienus altered the internal organization of the cohorts, there is at least, it may be claimed, no other period of Roman History to which the fivefold division of the cohort can belong. The reduction of the number of centurions from 60 to 50 was, we may conjecture, a consequence of the removal of senators from the higher commands. Successors had to be found not only for the *legati* but also for the *tribuni laticlauii*, and it was from the ranks of the centurions that these were recruited. At the same time with the growing barbarization of the army the problem of finding suitable men for the centurionate was becoming increasingly difficult, and Gallienus may have decided to tide over the crisis by a reduction in the number of centuries in the legion and a reorganization of the nine junior cohorts upon the model of the first.

## APPENDIX.

In the preceding article the account given by Vegetius of the strength of the cohorts and their subdivisions into five centuries apiece has been accepted as historical. Vegetius himself, it is true, was no military specialist, and sometimes commits himself to statements of detail that cannot possibly be right. The titles, for instance, and the order of seniority which he ascribes to the centurions of the first cohort (and perhaps, too, the relative strength of its centuries) are a confusion, and conflict with his own description of the system of promotion<sup>2</sup>. But the main points in his account of the internal organization of the legion appear to be consistent, and give the impression of having been derived from an authoritative source. Let us consider this claim in some detail.

Vegetius gives us the following account of the organization of the cohorts<sup>3</sup>:

Cohort I: 1,105 *pedites*, including 5 centurions; 132 *equites*, including 4 decurions.  
Cohorts II-X: 555 *pedites*, including 5 centurions; 66 *equites*, including 2 decurions.  
Total: 6,100 *pedites*, 730 *equites*, 55 centurions.

The arithmetic has clearly gone astray. The total number of *equites* should be 726 and of centurions 50. But the mistake about the centurions is explicable: Vegetius overlooked the fact that the first cohort, although *milliaria*, had only 5 centurions. So far, it is true, there is little to substantiate the claim of consistency; but there is further evidence. In II 25 Vegetius tells us that each century had a *carroballista*,

<sup>1</sup> This date has also been suggested by E. Stein in a note in his *Gesch. d. spätöm. Reiches*, p. 92.

<sup>2</sup> II 8.

<sup>3</sup> II 6.

that 11 men were detailed to take charge of it, and that in a legion there were 55 *carroballistae*. At first sight the total figure looks like a repetition of the mistake about the total number of centurions in a legion. I believe that it is not. Vegetius has merely omitted to say that in the first cohort, which was *milliaria*, each century had 2 *carroballistae*, a number consistent with his assignment of 132 *equites* to the first cohort, in contrast with the 66 belonging to each of the other cohorts. This interpretation may be confirmed by an analysis of the figures already quoted for the total strength of each cohort. Cohorts II-X have each 500 foot-soldiers, including 5 centurions, and 5 × 11 men detailed for machine-gun work—total, 555 men. Cohort I is twice the size of any other cohort, with the exception that it has 5 and not 10 centurions. It has, therefore, 995 foot-soldiers, including 5 centurions, and 5 × 22 machine-gunners—total, 1,105. Thus the totals given by Vegetius for each cohort are not inconsistent with his details about the number of centurions (allowance being made for an error in the total) and of *carroballistae*. There is, however, one difficulty. Vegetius says that in addition to the *carroballistae* there were 10 *onagri*, one to each cohort, but tells us nothing of the troops in charge of them. Two explanations are possible. Either the first century of each cohort provided a section of men for this work or in the first century of each cohort an *onager* took the place of a *carroballista*. If the latter solution is adopted, then the 10 *onagri* must be included in Vegetius' total of *carroballistae* (i.e. 45 *carroballistae* and 10 *onagri*).

I can find no good reason for maintaining that Vegetius or his source invented this organization of the legion; it seems to me rather that it is confirmed by later history. In the army of Constantine a *legio comitatensis* or *palatina* numbered 1,000 men, commanded by a tribune, and such legions were, to begin with at any rate, *nexillationes* from frontier-legions<sup>1</sup>. There can be little doubt that these new miniature legions consisted of either a *cohors milliaria* or two *cohortes quingenariae* of a frontier-legion. If then we subtract the machine-gunners from the strength of the cohorts as recorded by Vegetius, we find that a *legio comitatensis* or *palatina* corresponds either with the first cohort or with two junior cohorts of the *antiqua legio*, while the officer in command of the new legion has the same title as that given by Vegetius to the commander of his first cohort.

Nischer, on the other hand, believes that Vegetius' account contains very serious inaccuracies, and offers an entirely different solution. The basis of his reconstruction is that each cohort, including the first, had 6 centuries of 480 men each. He denies the possibility of the first cohort being *milliaria*, and assumes that Vegetius has assigned to it only 5 centurions, because he mistook the 2 *primipili* for 1 centurion. The superior strength of the first cohort he explains by supposing that it contained 5 centuries of veterans. This hypothesis he maintains by a novel interpretation of the list of arms (30 *antesignana* and 14 *postsignana*) which was found in an *armamentum* at Lambaesis<sup>2</sup>. He argues that a similar stock was kept in the armoury of the first maniple of each cohort, so that the total number of the veterans was 440, who, on being recalled for service, were divided into 5 centuries of 88 men apiece and attached to the first cohort. The organization of the legion is then reconstructed as follows:

Cohorts I-X: Staff of cohort ...	...	...	...	9
6 centuries of 80 men ...	...	...	...	480
11 gunners for 1 <i>onager</i> and 5 <i>carroballistae</i> ...	...	...	...	66
				<hr/>
				555
				<hr/>

<sup>1</sup> Grosse, *op. cit.*, p. 34.

<sup>2</sup> *Mém. de l'Acad. des Insers.* XXXVIII, 1909, p. 259.



<i>Vexillum of veterans</i> : Staff of <i>uexillum</i> ...				...	...	9
5 centuries of 88 men ...				...	...	440
11 gunners for 5 <i>carroballistae</i> ...				...	...	55
						<hr/> 504
						<hr/>
<i>First cohort of Vegetius</i> : Staff of legion ...				...	...	46
First cohort ...				...	...	555
Veterans ...				...	...	504
						<hr/> 1,105
						<hr/>

I do not propose to examine this theory in detail : its improbability may perhaps be apparent from the following observations :

1. One of the main arguments used by Nischer for maintaining the impossibility of the first cohort being *milliaria* is that there is no reference to it elsewhere in literature. This is untrue ; Hyginus tells us ' *cohors prima duplum habet numerum.*'

2. The first cohort was not divided as Nischer claims into six centuries. An inscription dated 253 A.D. shows, as we have seen, that the first cohort, in contrast to the remaining nine, had five centuries. Again from the early Principate the senior *primus pilus* was not in command of a century, but served on the staff. Consequently Vegetius' assignment of five centurions to the first cohort is in accord with the evidence of epigraphy and is not the product of a mistaken identification of the two *primipili*.

3. The theory about the veterans is unconvincing. In the first place, as Nischer himself recognizes, there is no trace in the second century of regiments of reserves, the probability being that they ceased to exist when service under the *aquila* was made coterminous with service in the *auxilia* (i.e. 25 years)<sup>1</sup>. Secondly, when these regiments did exist they had an organization separate from the legion with a *uexillum* of their own ; in peace time they were commanded by a *curator*<sup>2</sup>, in war by a centurion<sup>3</sup>. Thirdly, the theory that the veterans' arms were stored in the armouries of the first *maniple* of each cohort is inapplicable to the organization of the *antiqua legio* ; for in Vegetius *maniple* means *contubernium*.

In general Nischer's interpretation of the evidence is not so much a reconstruction of Vegetius as an original contribution upon the internal organization of the legion. As such it would seem to present almost as many difficulties as it professes to solve.

H. M. D. PARKER.

MAGDALEN COLLEGE, OXFORD.

<sup>1</sup> Domaszewski, *Die Rangordnung*, p. 80.

<sup>2</sup> *Dess. I.L.S.* 2338.

<sup>3</sup> *C.I.L.*, XIII 8276.

## DIMINUTIVES IN AUGUSTAN POETRY.

IN the course of his dispute with Conington on the comparative merits of Catullus and Horace,<sup>1</sup> Munro taxed the Augustans with having made the lyric of the heart impossible in Latin by their virtual exclusion of diminutives from the language of poetry; and, whether that is the result or no, the general fact that diminutives are rare in the serious poetry of the Augustan age is well known. The details, however, are less easy to come by. Stolz (*Hist. Gr. d. lat. Spr.*, p. 574) and Stolz-Schmalz (*Lat. Gr.*, p. 834) devote a few unilluminating lines to the *Stilistik* of diminutives: otherwise the grammars and the treatises on diminutives known to me<sup>2</sup> concern themselves only with forms and meanings. Except for a note by Professor Housman which, at 4. 927, sets out Manilius's diminutives, I know of no collections for any Augustan poet, and it is perhaps worth while therefore to state the facts. I have not indeed read through Augustan poetry for the purpose, but for some time past I have been in the habit of noting such diminutives as I have come across in the course of reading, and these lists I have now checked and amplified from the indexes to the authors concerned. My lists are probably not complete,<sup>3</sup> but I hope they are sufficiently near it to present a true picture of the position.

The collector of diminutives is met at the outset by a difficulty. Latin diminutive nouns in their simplest form represent what I will call their positives plus an adjective of size or quality—*agellus*, a small, or poor, or favourite field; *fraterculus*, a young or dear brother. There is, however, a second class of diminutives which have taken to themselves special meanings and risen to positions scarcely dependent, except etymologically, upon their positives; no mere adjective attached to *os* will give it the meaning of *osculum* or of *oscillum*. And, third, there are words in Latin which are suspected of being diminutives though their positives are no more to be found. Since the common diminutive terminations *-lus*, *-ulus*, *-culus*, together with their feminines and neuters, are also common suffixes in the formation of non-diminutives, in the second and third classes above mentioned etymologists frequently differ as to whether the termination indicates a diminutive, and still more frequently conceal their opinion on the point. *Iugulum* might be the diminutive of *iugum*, or it might be a parallel formation from the same stem: *capillus* and *uilla* are sometimes, but not always, regarded as diminutives of *caput* and *uicus*<sup>4</sup>: *stella* may be a diminutive, or again it may not. Many of these dubious words have no synonyms, and are therefore unavoidable if the object is to be mentioned at all. They are not of much consequence to this enquiry, the results of which would merely be obscured by the inclusion of such words as *macula*, *scapulae*, *scintilla*, *sella*, on the chance that they may be diminutives. I have, however, admitted some words of the kind, and marked them in my lists with an asterisk to indicate that their diminutive character is uncertain; but I have felt doubtful how to choose among them, and should perhaps have been stricter in my exclusions. I have excluded entirely another class of diminutives which force themselves upon the poet however reluctant he may be—proper names, whether of persons (e.g. *Regulus*) or places (e.g. *Albula*).

With this qualification the lists which follow contain in tabular form all the diminutives known to me in Virgil, Horace, Tibullus, Propertius, Ovid, and Manilius. The last I include since his first book at any rate was written in Augustus's lifetime, and since Professor Housman's list made it easy to do so. Horace's *Satires* and

<sup>1</sup> *Criticisms and Elucidations*, p. 235: cf. E. E. Sikes, *Roman Poetry*, p. 232.

<sup>2</sup> H. Kessler, *Die lat. Demin.*, Hilburghausen, 1869; G. Mueller, *de ling. Lat. demin.*, Leipzig, 1865; L. Schwabe, *de demin. Gr. et Lat.*, Giessen, 1859.

<sup>3</sup> Especially in Ovid, for Lemaire's index, on which I am largely dependent, is not free from omissions.

<sup>4</sup> The change of gender is not a certain criterion: see Kessler, p. 6.

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corolla  
\*culullu  
\*curricu  
fabella  
facula  
fiscella  
flabellu  
flagellu  
formulu  
\*iugulum

<sup>1</sup> uermi  
think, the

*Epistles* I have excluded since their vocabulary is in the main that of prose, the *Carmen Saeculare* since it contains only *puella*, Grattius because his diminutives are unimportant.<sup>1</sup> In the arrangement of Ovid's works I have grouped the *Amores*, *Ars*, *Remedia* and *Medicamina* together as nearly related both in theme and date; the *Tristia* and *ex Ponto* seemed just worth distinguishing, and the *Ibis* has therefore come by a column to itself, which it ill deserves. The last six epistles of the *Heroides* I have distinguished from the first fifteen since their authenticity is in dispute and they contain a couple of diminutives not otherwise used by Ovid.<sup>2</sup> Of other dubious or spurious works I have excluded the Virgilian appendix, though I shall have a word to say of it presently; also, from Ovid, the *Halientica*, *Nux*, and *Consolatio ad Liniam*. These three poems contain very few diminutives, and, except for the fish-name *sparulus* in the first, none not otherwise used by Ovid. The pseudepigrapha of Tibullus, of which the date is less uncertain, I have included in the figures for Tibullus; these contain one or two diminutives not used in the first two books, but nothing for which there is no other Augustan evidence. The letter *p* means *passim*; in the case of *capillus*, *osculum*, and *puella* I have written the word at length under Ovid, not meaning that these nouns are common in all his writings, but that he obviously uses them without scruple whenever they are needed.

SUBSTANTIVES	VIRGIL			HORACE		TIB.	PROP.	OVID							MAN.	
	B.	G.	A.	C.	Ep.			Her. 1-15.	Her. 16-21.	Am. A.A. Rem. Med.	Fast.	Tr.	ep.	Ib.	Met.	
agellus	I										I					
ancilla				I		I		I	I	13	I	I		I		
ancillula										I						
*anulus										6	I		2			
*armilla										I	2					
articulus							I	2		I			I		I	4
asellus, -a		I					2			5	9				2	
auricula							I									
*auunculus			2													
bucula	I	2														
calculus		I								2		I			2	
camella											I					
cancellus										I						
capella	13	2		I	I	I			I	2	3		I		6	I
*capillus		I	I	8	2	p	p				passim					4
capreolus	I															
castellum		I	I									2				
*catulus, -a	I	3	I	3			I	I		2	I				3	2
circ(u)lus		I	2									I			I	II
corolla							4									
*culullus					I											
*curriculum			I	I								I				
fabella						I										
facula																
fiscella	I					I					I					
flabellum																
flagellum		I	4	I	I										I	I
formula																
*iugulum		I	4					3	2				I	I	13	I

<sup>1</sup> *nermiculus* (387), for which he apologizes, is, I think, the only certain specimen.

<sup>2</sup> *Her.* 15, disputed on other grounds, has none peculiar to itself.

SUBSTANTIVES	VIRGIL			HORACE		TIB.	PROP.	OVID							MAN.	
	B.	G.	A.	C.	Ep.			Her. 1-15	Her. 16-21	Am. A.A. Rem. Med.	Fast.	Tr.	ep.	Ib.	Met.	
labellum	I						I		I	6						
lapillus		I					3			3					5	I
lectulus							2	I				I				
libellus					I	2	7	I	I	10		23	13	4		
loculus										I	I					
modulus																I
muliercula					I											
munusculum	I															
ocellus						I	18	2		14	I					
oscillum		I														
osculum		I	4	4		II	14				passim					3
palliolum										I						
palmula				I												
papilla			2				4			4						
parmula					I											
particula					2											
patella											2					
puella	4	4	2	10	3	p	p				passim					p
*pullus		I			I											
puluillus					I											
pupula					I					I						I
quasillum						I	I									
reticulum										I						
sacellum	I						2				2					
sagulum			I													
*scirpiculus							I									
scriptulum										I						
scrupulum										I						
sigillum										I						
*spiculum	I	2	17	2			2			4	2		5	I	5	2
*stella		5	7	5		I	2			3	12	4		2	7	p
*stipula	I	6				I				2	7	I			2	
surculus		I														
tabella					I	3	5			26	4	3	2		7	
tigillum						I										
uersiculus					I				I							
*uilla	I			2	I	2					I		I		2	
uirgula																4
*ungula		I	4		I						2	I	2		4	
uocula							I									
ADJECTIVES																
bellus						3				I						
gemellus <sup>1</sup>	I							4			3				3	
luteolus	I															
*masculus	I			I	I		I			I						5
nouellus	I					I				3		2				
paruulus			I			I	2									I
pusillus										I						
quantulus <sup>2</sup>										2	2		I			I
uetulus				2												
umidulus										I						

<sup>1</sup> Add *gemellipara*, Met. 6. 315, Fast. 5. 542.<sup>2</sup> The Ovidian instances are all + *cunq̃ue*.



This mixed bag will submit to some classification :

1. Doubtful diminutives of which no positive exists :

*pullus, stella*. This class, as has been said, it would be easy to swell.

2. Diminutives which diverge in sense from their positives :

*articulus, calculus* (where used on the *abacus* or for voting), *camella, castellum, circulus* (=necklet or diadem, as in Virgil), *flabellum, lapillus* (where used for games or voting; *A.A.* 3. 365, *Tr.* 2. 481, *Met.* 15. 41, 45), *loculus, oscillum, osculum, papilla, particula, pupula* (of the eye), *sacellum, scriptulum, scrupulum, stipula, surculus, uirgula*. Also some whose diminutive origin is doubtful: *anulus, armilla, auunculus*,<sup>1</sup> *capillus, iugulum, spiculum, uilla, ungula*. To this class belong also the adjectives *bellus* and *quantulus*.

3. Diminutives which had ousted or were ousting their positives :

*ancilla, asellus, calculus* (when used in games), *cancellus, capella, circulus* (in astronomy), *flagellum, formula* (legal), *puella*; and the adjective *pusillus*.<sup>2</sup> Possibly *palmula* should be added, for where Catullus had used both positive and diminutive (64. 7, 4. 4) Virgil's choice of the diminutive is odd; but the words are too uncommon to decide the point. *Masculus*, a dubious diminutive, cannot strictly be said to belong here, but in nine of its ten appearances *mas* is hardly an alternative, and in the tenth (*Prop.* 2. 15. 28) it is used as a substantive.

4. Diminutives denoting articles of dress or of household use :

From class 2: *anulus, armilla, camella, circulus, flabellum, loculus*. Add to these: *corolla, culullus, fiscella, lectulus, palliolum, patella, puluillus, quasillum, sagulum, scirpiculus, tabella*. Of the last none except *tabella* is at all common,<sup>3</sup> but the number of diminutives with such meanings catches the eye and seems unlikely to be accidental. In some cases it is possible that the diminutive has acquired by daily use a meaning distinct from its positive<sup>4</sup>; in others that familiarity has robbed it of some of its diminutive colour. Another small group of diminutives denoting objects used in games may be mentioned here: *reticulum* (*A.A.* 3. 361), *tabella* (*A.A.* 3. 365, *Tr.* 2. 481), and, from classes 2 and 3 above, *lapillus, scriptulum*.

5. Diminutives denoting parts of the body :

*auricula, labellum, ocellus*. *Articulus, capillus, papilla, pupula* have already been mentioned. Again it seems likely that daily use may have deprived the diminutive of some of its force.<sup>5</sup>

6. Diminutives denoting young animals :

*bucula, capreolus, catulus*. *Pullus* is in class 1; *asellus* and *capella* do not belong here in meaning. *Catulus*, which is probably not diminutive in form, was

<sup>1</sup> *Auunculus*, whether diminutive or not, was by some thought below the dignity of Epic; *Serv. ad Aen.* 3. 343.

<sup>2</sup> On *asellus* see *Philol.* 34. 153, *C.Q.* 24. 11. *Capella* is the ordinary Latin, at any rate in poetry, for the females of the domestic flock whatever their age or size; *capra* is used of Amalthea, on earth or translated, and of wild animals (*A.* 4. 152). Virgil twice uses *capris* of goats in general, the species, and the *Thesaurus* assumes it to be feminine. *Circus* in the astronomical sense of *circulus* seems not to occur between Cicero and the fourth century. Of *flagrum* the only Augustan example known to me is *Livy* 28. 11. 6 (at *Met.* 4. 367 *flagellum* means the arm of a polyp). *Forma* for *formula* seems only late Latin.

<sup>3</sup> *Tabella* has many uses, but much the commonest in these passages is *tabellae* in the sense

of documents or writing tablets; in this sense it seems to be supplanting *tabulae* and to be entering class 3. In the sense of *fan* (*Am.* 3. 2. 38, *A.A.* 1. 161) *tabula* is not in use.

<sup>4</sup> E.g. *lectulus* at *Prop.* 4. 8. 35, *Tr.* 1. 11. 38, seems to mean couch rather than bed; *palliolum* at *A.A.* 1. 734 hood rather than cloak.

<sup>5</sup> As to classes 4 and 5 it may be noted that in English a number of words ultimately diminutive in origin denote articles of wear (e.g. *bracelet, chaplet, corslet, doublet, gauntlet*), and one or two, articles of daily use (e.g. *goblet, mallet, naphin, pannikin, skillet*). Among parts of the body *buttock, gullet, knuckle* are the only examples which occur to me. I doubt if any of these are felt as diminutives: they are, however, further from their positives than most of the Latin words in the two classes.

	MAN.
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regarded by the ancients<sup>1</sup> as, in sense at any rate, the diminutive of *canis*,<sup>2</sup> but it is freely used of other animals and even of reptiles (*G.* 3. 438). Bentley's *haeduleae* at *C.* 1. 17. 9 would add another diminutive in this class. English analogies (e.g., *duckling*, *gosling*) might suggest here also some fading of the diminutive colour.

7. Diminutive adjectives whose positives already contain the idea of a diminutive:

*novellus* (in the sense of *young*), *parvulus*. *Pusillus* appears also in class 3. In some places (*B.* 1. 14, *Her.* 6. 121, 143, *Fast.* 2. 413, *Met.* 6. 712, 9. 454, 11. 316) *gemellus* also partakes of this idea. (At *Met.* 11. 316, *Her.* 13. 61 it is a noun.)

Fundamentally, no doubt, the reason why Augustan poets avoided diminutives was that their use belonged to the speech of common life and they were felt to be too intimate or too vulgar for high poetry. It will be noted, however, that in the first three classes set out above—in half, that is, of the whole total—this objection no longer attaches to the diminutives as such. Since their positives have ceased, for one reason or another, to be available as synonyms, though the diminutive form remains, the diminutive sense is lost, and the colouring, at the least, faded. They are in much the same position as such English words as *baby*, *billet*, *catkin*, *cutlet*, *donkey*, *gimlet*, *hamlet*, *puppet*, *tablet*, which most of us use, I imagine, with little or no consciousness that we are using diminutives at all. I have suggested that the same may be to some extent true, though for a different reason, of the words in classes 4, 5, and 6, or of some of them.

A poet's rejection of a word from his vocabulary naturally does not establish that it is, or is felt to be, diminutive, but it is plain from their rarity that to many words even in my first three classes some objection exists; and in some cases the existence of synonyms or partial synonyms enables us to measure the objection. *Capillus* and *puella*, for instance, are used quite freely by the elegiac poets but not freely by Virgil. In the *Aeneid*, *capillus* occurs once to *caesaries* four times, *coma* in this sense twenty-three, and *crinis* twenty-six; *puella* twice to *uirgo* (a partial synonym, but metrically less convenient) forty-seven times. Evidently, therefore, Virgil feels an objection to these words, and his evidence, so far as it goes, supports the view that *capillus* really is a diminutive.<sup>3</sup> On the other hand the freedom with which he admits to the *Aeneid* *spiculum*, a word with easy synonyms, suggests that this word, whether really diminutive or not, was not felt by him to be so. Again, diminutives of class 6 might be expected to occur with some frequency in Virgil in view of the subjects of the *Bucolics* and *Georgics*. How rare their appearance really is may be seen by comparison with *agnus* -a (13), *haedus* (13), *iuvencus* -a (42), *vitulus* -a (13), which are words of similar meaning but not of diminutive formation.

The words in my lists not hitherto mentioned are: *agellus*, *ancillula* (*Rem.* 639; the one double diminutive in the collection unless *oscillum* is such<sup>4</sup>), *calculus* in the sense of *pebble* (*G.* 2. 180), *curriculum*,<sup>5</sup> *fabella*, *facula* (*Prop.* 2. 29. 5; carried by a *turba minuta* of cupids), *lapillus* in the senses of *pebble* and *gem*, *libellus*, *modulus* (*Man.* 5.

<sup>1</sup> Varro *L.L.* 9. 45, Charis. *Gr.* 1. 94.

<sup>2</sup> The true diminutive *Canicula*, used twice by Horace and Manilius and once by Ovid, I have excluded as a proper name; since, however, the alternatives *Canis* and *Sirius* were open and preferred by Virgil, it perhaps deserves mention.

<sup>3</sup> Cat. 25. 1 *mollior cuniculi capillo, vel anseris medullula vel imula oricilla* possibly suggests that Catullus also felt *capillus* to be diminutive. Other less conclusive figures from the *Aeneid* are, *ancilla* 0 (X *famula*, *ministra*, *serua* 8 in all: *stella* 7) (X *astrum* 20).

<sup>4</sup> On the proposal to read *uillulae* at Hor.

*C.* 3. 4. 10 see *J. Phil.* 17. 313.

<sup>5</sup> I include this word because in one sense it was thought diminutive by ancient grammarians (e.g. Charis. *Gr.* 1. 77), who distinguish a diminutive *curriculus*, *car*, from *curriculum* in other senses. If they are right, *C.* 1. 1. 3, *Tr.* 4. 8. 36, deserve a place in my lists, *Aen.* 8. 408 not. Q. Cic., l. 16 (Baehrens, *F.P.R.*, p. 316), proves, however, that the word for *car* is neuter, and the presence of *curriculum* at *C.* 1. 1. 3 and in the verse of M. Cicero, who avoids diminutives, makes it improbable that it is a diminutive at all.

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<sup>1</sup> Libell  
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<sup>2</sup> E.g.  
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325; *tune*), *muliercula*, *munusculum*, *parmula* (Hor. C. 2. 7. 10; playful), *sigillum*, *tigillum*, *uersiculus*, *uocula*, and the adjectives *luteolus*, *uetulus* (Hor. C. 3. 15. 16, 4. 13. 25; two taunts to ageing beauties), *umidulus*, and some cases of *gemellus* and *nouellus*. Of these none but *libellus* is at all common,<sup>1</sup> and most are singletons. The most surprising is *muliercula*, which occurs, together with *uersiculus*, in the extremely unlyrical eleventh *Epode*; the still less lyrical eighth supplies *puluillus* and Horace's example of *libellus*. So far as Horace is concerned these words have no claim to be admitted to serious poetry.

The picture disclosed by these tables requires no long commentary. They present Virgil as extremely reluctant to admit diminutives, admitting practically none outside my first three classes, and, whether by reason of the theme or of the date or of both, stricter in the *Aeneid* than in the *Bucolics*. Propertius too seems to stiffen a little, for *corolla* and *ocellus* are confined to his first two books, *lapillus* and *libellus* to the first three. Horace is already so sparing of diminutives in the *Odes* that an increase of strictness is hardly to be discerned, but, apart from *puella*, the fourth book and *Carmen Saeculare* contain only *uetulus* (13. 25). *Puella* and *capillus*, to which Virgil appears to feel some objection, are freely admitted to the other writers. Tibullus, Propertius, and Ovid show *tabella*, in elegiacs *libellus*, and, for a time, *ocellus*, also promoted to respectability; by the intimacy of their themes also they allow themselves some diminutives which would seem strange in Virgil, but the total number is surprisingly small, and in hexameters neither Ovid nor Manilius can really be called less strict than the *Aeneid*.

Let us look back for a moment to Catullus and Lucretius. Catullus's vocabulary contains some fifty-odd diminutive nouns and half as many adjectives. Among them are Greek diminutives (*calathiscus*, *epistolium*), diminutive proper names (*Septimillus*, *Veraniolus*), and double diminutives (*oricilla*, *puellula*, *uillula* : *mollicellus*, *tantillus*, *tenellulus*). The diminutives are not, it is true, distributed with equal frequency over the whole of his poetry, being much commoner in hendecasyllables and the lighter lyric metres than in his hexameters and elegiacs, but they are not excluded from these. The *Peleus*, not to count *puella*, contains a dozen, and 99, an elegiac poem of fourteen lines, five. The general impression left by his practice is that their use depends, much as the choice of metre depends, on the mood and the subject, but that there was nothing in a diminutive *per se* which should exclude it from high and serious poetry. Lucretius tells a similar story. His numbers—rather more than thirty nouns, rather more than a dozen adjectives—are smaller, because his mood and theme invite diminutives less. Where, for once in a way, the subject favours them (4. 1160 sqq.), they are as thick on the ground as in Catullus; elsewhere he uses them freely when occasion serves, and sometimes plainly for mere metrical convenience<sup>2</sup>—and indeed Catullus probably did the like,<sup>3</sup> for the convenience to a dactylic

<sup>1</sup> *Libellus* is once or twice used in senses to which *liber* would be inappropriate—of letters (*Her.* 11. 2, 17. 143) and a programme (*A.A.* 1. 167): in the vast majority of examples, however, it is used of books of poetry, usually the poets' own, of single works or books (e.g. the *Medicamina*, *A.A.* 3. 206; the single books of the *Ars*, *A.A.* 3. 47; the *Remedia*, *Rem.* 1; a book of the *Tristia*, *Tr.* 5. 1. 1), or, more commonly, in the plural, of their writings. Since Propertius and Ovid both use *liber* of their own works also, it cannot be said that *liber* would be inappropriate in these places, but the diminutive is in Ovid very much commoner.

<sup>2</sup> E.g. *uermis*, *uermiculus* interchanged at 2. 871, 899; 3. 723, 728. Similarly *anulus* gives way to

*anellus* in metrically inconvenient cases (6. 1008, 1014, 1024, 1039). There is little or no evidence of similar freedom in the Augustans. *Fiscellam* (*B.* 10. 71), *fabellus* (*Tib.* 1. 3. 85), *luteola* (*B.* 2. 50), *umiduli* (*A.A.* 3. 629) are perhaps the most suspicious cases, but all admit of diminutive sense. The most that can be said is that in many places *liber* and *libellus*, *oculus* and *ocellus*, are equally admissible: and that in some other places a similar choice was open.

<sup>3</sup> This question is busily discussed by Mr. Platner in *A.J.P.* 16. 186 and by M. de Labriolle in *Rev. Phil.* 29. 276. Both provide lists of C.'s diminutives, and neither list is complete, though M. de Labriolle's includes *mentula* and *tremulus*.



poet of such adjectives as *aridulus*, *frigidulus*, *languidulus* (to instance those in the *Peleus* only) is obvious.<sup>1</sup>

The vocabulary of Lucretius and Catullus together contains (since a few diminutives are common to both) about eighty substantives and rather less than half as many adjectives. My collection from the Augustans contains sixty-nine nouns and ten adjectives. Of the nouns some thirty occur in Lucretius or Catullus or both; of the adjectives all but *luteolus* and *masculus*. The diminutive vocabulary of the two parties is therefore not coincident, and since the bulk of the poetry which I have examined is about six times that of Lucretius and Catullus together, a simple calculation will show approximately the number of diminutives to be expected in the Augustan poets if they had maintained towards such words the attitude of their predecessors. It is plain that in the interval between Lucretius and Catullus on the one hand, Virgil and Horace on the other, a very marked change has occurred.

In the case of the substantives this would seem to be a part of the differentiation of vocabulary into strata, whether by the deliberate choice of the poets or by increasing linguistic sensitiveness in their audiences, which has in the main left diminutives on the lower levels from which the poets do not and must not draw. In the case of the adjectives, where the shrinkage is still more remarkable, some further agent seems to be required to account for the facts, and a little investigation shows what it was. If Horace's hexameter verse were included in my figures, his total of diminutive nouns would be increased by between fifty and sixty; his adjectives, I believe, only by half a dozen,<sup>2</sup> of which most are admitted by other Augustan poets and occur in my lists. This might, of course, be an idiosyncrasy of Horace; that it is not is shown by the fact that a similar disparity of numbers between diminutive noun and diminutive adjective is to be found in Juvenal, Martial, and Petronius, and is larger in the prose writer than in the poets.<sup>3</sup> Diminutive adjectives therefore were not merely sinking in status; they were largely going out of use.

The settlement of a language into strata and the passage of words from one stratum to another are not in themselves remarkable; they probably occur in all languages in which literature attains to self-consciousness. What is remarkable here is the rapidity with which diminutive stock has fallen; for *Bucolics* and *Georgics*, *Epodes* and *Odes*, are separated from the deaths of Lucretius and Catullus by a generation or less. It seems likely that the speed with which the reform was effected may have been due in part to the fact that the Augustans were returning to the usage of older poets; or, in other words, that the surprising innovation was not the restricted use of diminutives in the second half of the century but the freedom of their use in the first. The evidence is perhaps insufficient to decide, but in favour of this view it may be said that in the serious Latin poetry of earlier date diminutives are not common. Ennius provides one or two, the tragedians about a dozen; and, among the fragments of Latin poetry, it is in the scanty remains of such poets as Laevius, Bibaculus, and Cinna that one first seems to discern something like the attitude to diminutives shown by Catullus. It might also be said that such an attitude would not be surprising in poets whose Greek models were largely Alexandrian, and that Cicero, in whose poetry diminutives are noticeably scarce,<sup>4</sup> is less likely to have been a precursor of Virgil than a belated follower of the older poets as against the *cantores Euphorionis*, by whom, on this hypothesis, we must suppose Lucretius to have been influenced. But however this may be, the change was almost complete in a genera-

<sup>1</sup> Lucretius, half of whose diminutive adjectives denote size or number, has, I think, of this shape only *umidulus* (4. 632: Lachm).

<sup>2</sup> I have noted only *gemellus*, *parvulus*, *pauperculus*, *pusillus*, *quantulus*, *tantulus*.

<sup>3</sup> The proportions are, very roughly, Martial

1:5, Juvenal 1:7, Petronius 1:9. In Persius and Phaedrus, whose diminutives are fewer, the numbers are about 7:20 and 5:30 respectively.

<sup>4</sup> *Eculeus* (fr. 42) is, I think, his only certain diminutive.



tion; and as there is little or no poetry which can be securely assigned to the interval in which it took place, we cannot hope to trace the process in detail. It may just be worth adding that of the longer poems in the Virgilian appendix the *Ciris* seems in this matter to follow the principles of Catullus; the others it would be possible to consider intermediate between Catullus and Virgil. These poems, however, are short, many factors contribute to a poet's choice of vocabulary, and I should not myself venture to pronounce such an opinion.<sup>1</sup>

I have not pursued this enquiry with much diligence among the serious poets of the Silver Age, but I know enough of Lucan, Valerius, and Statius to be aware that the restrictions of the Augustans were stiffened rather than relaxed by them. Statius admits a few diminutives to two of his hendecasyllable poems (*Silu.* 1. 6, 4. 9), but with that exception diminutives of any kind in these poets (and I fancy Silius agrees with them) are very rare indeed. Of the nouns that I have noticed all belong to my first three classes, and practically all are admitted to the *Aeneid*<sup>2</sup>; the only adjective known to me in the three is *tenellus* (*Silu.* 5. 5. 86). Even the bucolic poets, of whom more freedom might be expected, hardly go beyond my first three and my sixth classes.<sup>3</sup> That is not the end of the story, for later on the speech of poetry and that of everyday life were to approach one another again, and a fine harvest of diminutives might be gathered from such poets as Prudentius and Paulinus of Nola; but I cannot sketch the history of the rapprochement and do not even know whether the emperor Hadrian's orgy of diminutives is a harbinger or an isolated outburst.

A. S. F. Gow.

TRINITY COLLEGE, CAMBRIDGE.

<sup>1</sup> In the *Ciris*, *Copa*, *Culex*, *Dirae*, *Lydia*, *Moretum*, I have noted, in addition to *puella*, the following diminutives: *agellus* (D.), *araneolus* (Cu.), *asellus* (Co.), *capella* (Cu., Ci., D., L.), *capillus* (Ci.), *caseolus* (Co.), *casula* (M.), *corolla* (Co.), *hortulus* (Ci.), *labellum* (Ci.), *lectulus* (Ci.), *libellus* (D.), *mitella* (Co.), *nutricula* (Ci.), *ocellus* (Ci., L.), *osculum* (Ci., Cu.), *pistillus* (M.), *pupula* (Cu.), *recula* (M.), *spiculum* (Ci.), *stella* (Ci.), *tabella* (M.), *uaccula* (L.),

*uitacula* (L.): *aureolus* (Cu.), *virgidulus* (Ci.), *parvulus* (Ci., Cu., M.), *tabidulus* (Ci.).

<sup>2</sup> Exceptions are *musculus* and, if you choose to count it, *basiliscus* in Lucan.

<sup>3</sup> Exceptions are *modulus* (Buc. Einsid. 1. 24, 35), *rotulus* (Calp. 7. 51). From Virgil's *Bucolics* Calpurnius borrows *fiscella* and, in the next century, Namatianus, *labellum*. The last poet has also *vermiculus* (Auc. 28).

## NOTES ON THE TEXT OF SENECA'S LETTERS.

THE text of Seneca's Letters, despite the attention it has received from scholars in the last fifty years, still leaves much to be desired in a large number of places. It is a field in which emendations can be proposed (and judged) with rather more security than is often the case in classical Latin prose, because Seneca was a very prolific writer, exceeded only by Cicero and Livy in the bulk of his extant work. The absence of a special lexicon for this eminent author constitutes a genuine difficulty of course, but it can be overcome in part at least by a constant reading and re-reading of his works, not excluding the dramas. The following suggestions are based on a more intense application to Seneca than Quintilian would be likely to have approved.

*Ep.* 11, 1 (Hense<sup>2</sup>, p. 28): Nulla enim sapientia naturalia corporis [aut animi] uitia ponuntur.

The excision is due to Madvig. But Summers' note *ad loc.* (*Select Letters*, p. 164) shows clearly by reference to *Ira* 2, 2, 2 that Seneca differentiated, sometimes at least, between a *voluntarium animi uitium* and those faults of mind which *condicione quadam humanae sortis eueniunt*.

The references in the *Epp.* themselves on the subject are: 57, 3-4; 63, 1; 71, 27; 74, 31; 75, 12; 85, 6 sqq.; 94, 55 sqq.; 99, 15-16; and 106, 5. There is absolutely no harmony to be got out of these. The fact appears to be that Seneca *qua* philosopher pure and simple takes strong ground against the existence of any *naturalia animi uitia*, especially where, as in 85, 6 sqq., he is arguing against the position of another school; but when he views human experience without prejudice or the felt necessity of making out a case (57, 3-4; 63, 1; 99, 15-16 and 18), he is forced to recognize the existence of flaws of mind in the sage. Cf. especially 57, 4, where the weakness of even the sage's nature in the face of certain things is admitted. In 74, 31 these weaknesses are said to occur *non ex imperio animi*; but if there are occasions even in the experience of the sage where the mind lacks control, surely its very lack of control is a very serious *animi uitium*, as the *Ira* passage referred to above frankly admits. As Seneca shows such vacillation of view on the subject, it is surely a presumption to attempt to correct any single passage in one direction or another, especially against the MSS. consensus at that point. *Aut animi* should stand.

*Ep.* 27, 5 (Hense<sup>2</sup>, p. 90): Huic memoria tam mala erat, ut illi nomen modo Ulixis excideret, modo Achillis, modo Priami, quos tam bene [nouerat] quam paedagogos nostros nouimus.

The excision is due to Gronovius, whose principal argument is: Si tam bene nouerat, quare tam facile excidebant? nam cui tam facile excidebant, is numquam eos tam bene nouerat. But the obvious answer is that they dropped out easily because his memory was so bad; and in fact there are those of us with a memory reasonably good who find, with advancing years, very well-known names in literature slipping from our minds all too easily, although we know them as well as we know anything. And in any event, as Fickert points out, to excise *nouerat* is to overlook *modo* of the preceding sentence. The parvenu did not *always* forget Ulysses, Achilles, and Priam, but sometimes one and sometimes another. The pitifulness of his memory-lapses really lay in the fact that he did know the names perfectly well, *tam*

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*bene quam paedagogos nostros nouimus*. The subject for *nouimus* is not, as Gronovius would have it, *omnes liberali doctrina imbuti*, but simply *nos omnes*.

Incidentally Summers' observation (*Select Letters*, p. 27, f.n.), 'there is no reason why the parvenu should be so familiar with Homer,' is rather beside the mark. To know quite well the names of Hamlet, Romeo, and Shylock would not necessarily involve a very profound knowledge of Shakespeare.

*Ep.* 40, 1 (Hense<sup>2</sup>, p. 120): si imagines nobis amicorum absentium iucundae sunt, quae memoriam renouant et desiderium [absentiae] falso atque inani solacio leuant: quanto iucundiores sunt litterae, etc.

The excision of *absentiae* is due to W. Gemoll. *Absentiae* is, however, Seneca's way of repeating the notion *absentium* in accordance with a manner of speech found in Suetonius (cf. *Caligula* 55, where *uicinia* means *uicini*) and elsewhere in Seneca himself. In the *Epp.* these instances seem indisputable:

- 74, 23: *perfidia* for *perfidus amicus*, and *impietas* for *impii liberi*;  
90, 10: *seruitus* for *serui*;  
99, 6: *desiderii* for *desiderati amici*.

The abstract noun has a value either singular or plural according to the context. *Absentiae* is sound therefore in the present passage, and the phrase means: 'the sense of yearning for those who are absent from us.'

*Ep.* 48, 8 (Hense<sup>2</sup>, p. 145): Hic, cum quo ludis, timet; succurre, quidquid laque † ti res pendentium penis.

The corruption appears in much the same form in all the MSS., including Beltrami's Q. Hense<sup>2</sup> comments: *eiusdem imaginis esse laquei (aut laqueandi) et pendendi uerba adparet*. Lipsius had anticipated this idea.

In the preceding sentence four words indicating distress, but not necessarily its extremity, were followed by a specific instance implying impending death: *intentae securi subiectum praestantibus caput*. Probably it is here paralleled by a phrase descriptive of another capital punishment, hanging.

For *laqueati res* (LQ) I propose *laquei artioris*. The combination is well attested; see the *Thesaurus*, p. 720, s.v. *artus* (adj.) and note esp. Pliny, *Epp.* 2, 18, 2: *hos artissimos laqueos abrupam*. This passage and Seneca, *Dial.* 9, 10, 1, are confirmatory of Buecheler's *rumpens* for *ium penis*. Apart from that for *pendent* I read *pendet*.

'Breaking whatever of tighter noose is hanging' sounds decidedly like a phrase from tragedy, and receives striking confirmation from a comparison with Seneca's *Phoenissae* 148-9—

*Artis colla laqueis inseri*

Prohibebis?

Cf. also in his *Phaedra* 1086 the phrase *laqueo tenaci*.

*Ep.* 53, 9 (Hense<sup>2</sup>, p. 163): Exercet philosophia regnum suum; dat tempus, non accipit. non est res subsiciva; ordinaria est, domina est, adesse iubet.

*Adesse iubet* is Haase's conj., unnecessary, I think, and unrhymical, for *adest et iubet* of all the MSS. I suggest: *non est res subsiciva; ordinaria est. domina et adest et iubet*. All that this involves is the placing of a full stop between *est* and *domina* and reading *et* for the *est* after *domina*.

The word *ordinarius* is not, I believe, to be interpreted here as in passages like *Ep.* 110, 1 and *Benef.* 3, 28, 5, but rather as in *Ep.* 7, 4 and *Ira* 3, 31, 2, with the meaning 'regular,' 'according to routine.' This sentence is then, as I see it, an inverted comment on *dat tempus, non accipit* preceding. If philosophy were a thing for leisure hours only, she would have to accept such time as we chose to give her;

as she is the regular routine of life, at least for the aspirant *sapiens*, it is she who concedes time for occasional relaxation from her fixed round.

Then, as a symmetrical close to the sentence-opening, *exercet philosophia regnum suum*, comes *domina et adest et iubet*; 'as mistress of the house she is on the spot and issues her orders.' I believe that *domina adest*, which worried Haase, is satisfactorily commented on by the opposite phrase in *Marcia* 10, 6: *mancipiorum suorum neglegens domina*.

*Ep.* 108, 7 (Hense<sup>2</sup>, p. 515): *nec aliter concitantur quam solent Phrygii tibicinis sono semiuiri et ex imperio furentes*.

*Furentes* is the conj. in Erasmus<sup>1</sup> for the *fugientes* of B and the first hand of A; it appears to have been accepted without challenge since. But a close examination of the *locus classicus*, Catullus 63, seems to me to confirm the MS. reading. Note especially the passage v. 277 sqq. where, after the exhortation of Attis, there is a mad rout towards the house of Cybele—surely a case of *ex imperio fugientes*.

I question the soundness of *et*, however. *Phrygii* makes up with *semiuiri* the subject of *solent*, and in that case there is no logical justification for the *et*. If *et* is to be retained, it would seem necessary to treat *semiuiri* and *fugientes* as being both predicatives, and to translate: 'they are stirred as the Phrygian (devotees) are wont to be at the sound of the flute, unmanned and fleeing away at the word of command.' The Loeb translation does not face the difficulty involved in reading *et*.

WILLIAM HARDY ALEXANDER.

UNIVERSITY OF ALBERTA,  
EDMONTON, CANADA.

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## PLATO AND 'IMITATION.'

IN *C.Q.*, January, 1928, pp. 16 sqq., I examined afresh the two discussions of poetry as imitation which are found in Plato's *Republic*. I pointed out that Plato used the term 'imitation' (*μίμησις* and cognates) in two senses, a good and a bad. (The only kind of poetry which Plato excludes from his ideal state is that which is imitative in the bad sense of the term. He admits, and indeed welcomes, that kind of poetry which is imitative in the good sense (without discussing the question whether any such poetry is actually extant), and which he calls either imitative or non-imitative according as he is using the term 'imitative' in a good sense or a bad. The kind of poetry (briefly summed up in *Rep.* 607a as consisting of hymns to the gods and eulogies of good men) which is admitted into the ideal state is certainly imitative; and (which is the important point) it is in fact called imitative by Plato himself (e.g. in 397d τὸν τοῦ ἐπαικοῦς μμητὴν ἀκρατον)—of course in the good sense of the word. Now the tenth book begins by stating that the result of the earlier discussion had been the decision to exclude 'so much of poetry as is imitative.' The implication is obviously that the remainder will not be excluded. (Plainly it is assumed that such a remainder either exists or will be successfully called into existence by the ideal state.) But what are we to call that remainder, unless it be non-imitative in the sense (the bad sense) in which the word is here used? Clearly the poetry which is admitted is imitative in one sense and non-imitative in another sense. Clearly too the distinction between the two senses was not an unconscious one on Plato's part. Plato did not always choose to avoid verbal contradictions; why should he, indeed, since they have their uses in stimulating thought? But he was certainly alive to real self-contradictions; and the self-contradiction involved in calling the same kind of poetry imitative and non-imitative without distinguishing two meanings in the term would be far too flagrant to escape his notice. The distinction between the two senses of imitation is in fact, as I have argued, clearly, explicitly and deliberately made. The alleged contradiction (of which commentators have made so much) between the earlier and later discussion amounts to no more than a play upon words, and represents no real inconsistency of thought. No doubt Plato would have made things easier for the mechanically-minded reader if he had used technical terms to distinguish the two senses. But at the time of writing the *Republic* he had not yet descended to such devices.

(The kind of poetry (or painting or oratory or any other art) which is imitative in the good sense is that which imitates the ideal world. It uses 'the divine paradigm' (cf. *Rep.* 500-1). It represents its object (whether that object be a man or a bed or anything else, and also, I think I am justified in adding, whether it be imaginary or actual or partly one and partly the other) in the object's *real* aspect (cf. *Rep.* 597a), represents it, that is to say, as partaking in, or imitating, the true reality which is the ideal world. What it produces is not merely a copy of this or that concrete object as perceived by the deceptive faculties of sense, imagination and opinion, but a copy of the ideal realities which the object 'imitates' or 'partakes in' and whose 'presence' in the object constitutes the object's sole reality. This kind of art is therefore but once-removed from truth; it represents not the object alone or the ideas alone, but ideas plus object (*Rep.* 500-1). And this kind of artist must be a man who enjoys the vision of the ideas of justice, beauty, etc., so that he may embody them in his

work. He must be a true philosopher, a man of wisdom and (therefore) of virtue, one able to 'search out the nature of the beautiful' (401c) and 'stamp the image of the good upon his works' (401b).

That kind of poetry (or other art) which is imitative in the bad sense is that which copies more or less slavishly the external characteristics (appearance, words, gestures, etc.) of its object—those features which are perceived by, and as they are perceived by, sense and opinion divorced from knowledge. It lays hold merely on the unreal aspect of its object, and fails to express its inner meaning or essence. It merely 'lays on some colours' of the material things or the qualities of mind which it represents, knowing and caring nothing about them save how to imitate their externals (601a). It therefore produces something twice-removed from truth (596 sqq.). The artist who is imitative in this condemnatory sense of the word is characterized by ignorance. He has never beheld the vision of the ideal realm. He has never been trained by dialectic to see below the surface to the true nature of objects.

Thus it is clear that Plato's treatment of imitation is really a special application of his doctrine of the opposition of 'opinion' and 'knowledge.' Just as the so-called virtue which is divorced from knowledge is merely 'a kind of scene-painting,' i.e. an illusion and a sham, 'slavish and having no soundness or truth in it' (*Phaedo* 69b), a spurious imitation of genuine virtue (as opinion is of knowledge), so poetry which is imitative in the bad sense is sham poetry; the poet who writes it is a mere 'wand-bearer,' whereas the genuine poet is not merely a wand-bearer but genuinely inspired (by knowledge of the ideas) and has the root of the matter in him (cf. *Phaedo* 69c.)

Such, then, is Plato's doctrine both in the *Republic* and elsewhere; and the earlier and later discussions in the *Republic* are thoroughly in accord with each other. Favourable comments by distinguished scholars on my previous article dealing with this subject encourage me to offer now the following considerations in support of its conclusions.

In the first place, the word 'imitation' naturally tends to bear a meaning very much akin to what I call the bad sense of the term in Plato. (I need not encumber my article with a long list of examples, for the point is obvious enough.) I do not, of course, claim that this was always the flavour of the word. I do not, for example, know (though I have my suspicions) whether the Pythagoreans (referred to by Aristotle<sup>1</sup> as at least earlier than Plato) meant any particular disrespect for 'things' when they said that 'things exist by imitation of the numbers'; and it is clear that Aristotle used the word (e.g. in his definition of tragedy) without any contemptuous nuance. I merely contend that with the Greeks as with us ('Beware of imitations') 'imitation' could naturally be used to imply (saving the context) a merely external, and sometimes a deceptive, resemblance, and not a real and internal sameness or similarity. When art was called imitation, it was (at least before Aristotle) ordinarily so called with reference to the merely outward and apparent resemblance between a work of art and the object represented; in fact, the greater the deception the better the art was popularly supposed to be. Hence the surprise of Parrhasius when Socrates, according to Xenophon's story, applied the word (with synonyms) to the expression not of the material but of the invisible: a painting (Socrates said) could imitate the character of a man's soul and not merely his external appearance. The fact that this doctrine was novel shows that the use of the word (as applied to art) was equally novel. Similarly in the conversation with Cleitophon the sculptor, which Xenophon also records, Socrates makes what is evidently a somewhat novel point, that one must 'imitate' (*προσεκάζειν* = *ἀπομιμῆσθαι*, as the context shows) not merely the actions of the body but also the activities of the soul.<sup>2</sup>

These passages from Xenophon help to explain why Plato uses the term *μίμησις*

<sup>1</sup> *Met. A.* 6, 987b, 7 sqq.

<sup>2</sup> *Xen. Mem.* III. 10, 1 sqq.

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and its cognates in a good sense 'with reserve as a kind of metaphor,'<sup>1</sup> and why he did not expect the reader at the beginning of the tenth book of the *Republic* to find any difficulty in understanding the word in its bad sense. There was no need for any note as to the normal implications of the word; for it was the good sense, not the bad, which was felt to be in some degree abnormal and paradoxical. One may indeed point to many passages in Plato's own works to show that the term naturally lent itself to the condemnatory meaning, the meaning which implies mere 'miming,' mere external copying, the production not of something really like the original (in the sense of reproducing its 'form' or essence or inner meaning) but merely of something which appears like it. For example, in the *Gorgias* 513b Socrates says that in order to please the demos one must be not a mere μιμητής of the demos but αὐτοφύως ὁμοίος; that is to say, it is not enough for one to copy its external characteristics while remaining internally unlike it and at variance with its principles: no, one must be radically like it, think its thoughts, accept its ideals, believe its principles with one's very soul. Similarly in the *Politicus* 300d it is said that when men of skill and scientific knowledge imitate (in the good sense) the true polity (the ideal archetype), the resulting constitution is not an imitation but the genuine article; that is to say, μίμημα has the nuance of counterfeit, and such words do not naturally lend themselves to mean the successful embodiment (whether by artist or by statesman) of the ideal in concrete form, though they have to bear at times this latter meaning (the good sense) also—in contexts which, in my opinion, leave no room for misunderstanding.

Again, although in working out Plato's treatment of imitation I originally used no dialogue other than the *Republic* itself, in which we find the fullest treatment of poetry under that aspect, it nevertheless deserves to be pointed out that my conclusions are confirmed by all the other dialogues which touch upon the subject. I have already dealt briefly with the relevant portions of the *Gorgias*, *Phaedrus*, *Symposium* and *Laws*. Here I wish to draw attention to two passages of the *Sophist* in which the two senses of the term 'imitation' (the literal, with its implication of mere outward and deceptive resemblance, and the metaphorical, involving a real, internal, formal and essential similarity) are explicitly distinguished. In the first passage (236abc) the art of imitation (εἰδωλοποιική = μιμητική) is divided into εἰκαστική, the art which produces 'that which is other but like,' and φανταστική, which produces 'that which appears to be like but is not.' (These correspond respectively to the good and bad senses of the term 'imitation' in the *Republic*.) And μίμησις—imitation in the literal sense, 'mimicry'—is the term naturally appropriated to that part of φανταστική where the creator of the appearance uses himself as the instrument of imitation, i.e., where the imitator makes himself appear like another person in body or in voice. (This is exactly the condemnatory sense of the word as used e.g. in *Rep.* 393c.)

The second passage, *Sophist* 265-8, arrives at what is virtually the same distinction in another way. The man who knows that which he imitates may practise

<sup>1</sup> As I wrote in *C.Q. loc. cit.*, p. 18, n. 1. They also explain why, though 'Plato consciously drew this distinction' (between true and false imitation), 'he did not emphasize his meaning' (cf. Sikes, *The Greek View of Poetry*, p. 77), at least in the *Republic* (for, as we shall see, he did emphasize it elsewhere).

The distinction between the two senses of imitation was seen (but not worked out clearly) by Finsler, *Platon und die Aristotelische Poetik*, pp. 19 sqq. It was also virtually seen by Adam, who, in view of the condemnation of only ἡ πρὸς ἡδονὴν ποιητική, 607c, and of τῇ τοιαύτῃ ποιήσει, 608a, pointed out (n. on *Rep.* 608a, 6) that 'there

is another sort of Poetry which Plato would not exclude.'

This citation reminds me to add that the refusal of commentators to regard the tenth book of the *Republic* as supplementary to, and consistent with, the second and third is the more remarkable in that Plato in 607b (in addition to 595a) refers back to the third book and his previous decision regarding poetry. Plato certainly saw no inconsistency between the two discussions. (More proof that in writing the tenth book he had not forgotten the third is afforded by 603c, which refers back to III. 399abc.)



imitation of either kind, i.e., he may represent mere externals or he may express the inner and essential characteristics of the object imitated. Mere external copying, however, will not greatly appeal to him; he will know that at best it is merely a form of amusement (cf. *Rep.* 396e, 2, 602b, 8; *Phaedr.* 276b, 2). The more meaningful form of imitation will be more to his taste. But the man who does not know the real nature of what he imitates can practise only the first kind of imitation—that which deals in outward and apparent resemblances exclusively. Accordingly the art of imitation may be divided into the imitation practised by those who know and the imitation practised by those who do not know. These correspond respectively (though with the reservation that even he who knows may indulge occasionally in imitation of the literal kind) with imitation in the metaphorical sense (the reproduction of inward realities) and imitation in the literal and primary sense (the representation of external appearances). In order to make clear this distinction between intelligent (or scientific) and unintelligent (or unscientific) imitation, a technical term (which might have made matters easier for the reader of the *Republic*) is devised in this passage of the *Sophist* for the imitation practised by the ignorant: *δοξομιμητική* (imitation of appearances); while the imitation which is practised by the man of knowledge is called *ἡ μετ' ἐπιστήμης ἱστορική τις μίμησις* (267de). *Δοξομιμητική* is further divided according as it is practised by men who are conscious or by men who are unconscious of their ignorance. Plato would probably have placed Homer in the second of these subdivisions. But, however that may be, Homer's art belongs to *δοξομιμητική*; for it is clear from the *Republic* that Homer belongs to the class of those who, having no knowledge but a mere opinion of some kind about justice and virtue, imitate their own notion of justice without knowing what justice is (*Sophist* 267c), and so make it appear that they are just. Those who are really just imitate (in the good sense), as is clearly indicated in 268d, not a mere notion about justice but justice itself, the idea of justice.

The above remarks, taken in conjunction with my previous article on the question, are, I think, an adequate defence against those who allege a disunity in Plato's thought on the subject of imitation. But the discussion also has a bearing at various points on certain current misconceptions, or what I believe to be such, regarding Plato's treatment of poetry. I take the opportunity of dealing with some of these.

In the first place it is clear that Butcher<sup>1</sup> drew far too rigid a distinction between the use of the word 'imitation' by Plato and its use by Aristotle. For Plato it does very often mean 'servile copying,' as Butcher said, i.e., the mere representation of sensible appearances. But it may also mean for Plato as for Aristotle the expression of 'universals.' Plato, no less than Aristotle, could have spoken of the 'imitation' of 'things as they ought to be.' With the passages already cited in support of this statement (such as *Polit.* 300cd on politics as imitations of the truth) compare, e.g., *Laws* 668b (imitation of the beautiful) and 817b (on Plato's own laws as an imitation of the best and noblest life, and as competing in that respect with tragedy at its best). The only difference is that for Plato the latter (the good sense) is still a somewhat metaphorical use of the word. It is a great pity that Butcher's remark is so often copied in a form which implies that Plato had no conception of poetry and art save as a slavish realism, a mechanical mirroring of the visible, audible, tangible world. This is in fact the conception of art which Plato did his best to refute; and in so far as he is hostile to Homer and artists of the Homeric type, his hostility is due simply to his belief that they based their works on this wrong conception. What Plato requires of painting, for example, is not that it should be photographic, not that it should slavishly reproduce this or that object, but that, just as the object copied is an imitation of the idea, so too should a picture be a direct (only once-removed)

<sup>1</sup> *Aristotle's Theory of Poetry*, c. II.

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*Op. cit.*  
<sup>2</sup> P. 23



imitation or expression of the idea or ideas in some way immanent in the sensible original, in order that, like that original, it may be capable of reminding us of the ideal world which is the true reality. Thus I am glad to agree with Mr. E. E. Sikes<sup>1</sup> when he says that the Platonic doctrine of art as imitation is the doctrine of 'the idealization of nature.' I am, however, unable to follow his next sentence, which states that for Plato 'sensibilia could not disclose Reality.' *Sensibilia* are imitations of Reality (*Rep.* 597-8). They remind us of it (*Phaedo* 74bc *et al.*). Nor can I agree that Plato 'did not foresee the answer of the neo-Platonists, that an artist may seek, through phenomena, the beauty which nature only in part reveals.' This 'answer' comes from such sources as Diotima's speech in the *Symposium*, a dialogue which, as Mr. Sikes apparently admits, tells against this criticism. Compare also *Rep.* 396, 401-2, 500-1. In another passage<sup>2</sup> Mr. Sikes says that Plato's 'heresy' was disposed of by the 'neo-Pythagorean' view 'that sensible things are symbols of divine ideas,' that 'the whole world is a symbolic poem, a revelation of some spiritual truth.' 'It followed that the poet, being inspired, was no mere copyist of nature, but the revealer of the non-sensible.' But in the first place, as I have already indicated, this neo-Pythagorean view was no novelty. It belongs to Plato himself; and these late writers probably took it from the *Timaeus*.<sup>3</sup> In the second place, Mr. Sikes, if I understand him aright, is here rather inconsistently ascribing to Plato a view of art very different from 'the idealization of nature.' It is the 'mere copyist of nature' whom Plato condemns in the *Republic*. He never said that 'an artist may seek' to be nothing more than that. In fact he requires the artist to seek to be a good deal more. It is his continual demand that the poet and every other artist should be 'a revealer of the non-sensible,' above all of beauty (*Rep.* 401d, *Symp.* 212a) and of truth (*Phaedr.* 277-8, *Rep.* 401-2, *Laws* 667-9). The neo-Pythagoreans and neo-Platonists had nothing to teach Plato on this subject.

At the very least Mr. Sikes has fallen into a far too frequent ambiguity in his use of the words 'the poet.' It is the same ambiguity as is found in the statement frequently made (Mr. Sikes of course does not make it) that Plato expelled 'poetry' or 'the poets' from his ideal state. If the statement refers merely to the actual poets whose works Plato knew, then it is probably true, though Plato commends various passages (and at least one poem) which they have written. But if it means (as it too often does) that there would be no poetry and no poets in the ideal state, then it is certainly false.

If then we take Mr. Sikes to mean that certain actual poets, such as Homer, were, according to Plato, mere copyists, mere imitators in the bad sense of the term, two questions arise. Firstly, how does this censure of particular artists and works of art mean that Plato had a low estimate of art as such and never thought of the view that 'an artist may seek, through phenomena, the beauty which nature only in part reveals'? The answer is that it means nothing of the kind; those who think that it does are merely assuming that Homer, etc., represent the ideal standard of artistic production, and that Plato's reasoned refusal to identify Homer with his ideal artist is something which they are entitled to ignore. Secondly, in what way did the neo-Pythagoreans and neo-Platonists answer Plato's attack on Homer? Neo-Platonic views of art appear to me quite in accord with Plato's doctrine. Both Plotinus and Proclus (whose views are conveniently summarized by T. Whittaker, *op. cit.*, pp. 90 and 272) recognize the distinction between the imitation which is once-removed and that which is twice-removed from the ideas. They differ from Plato only in taking a slightly more lenient view of the art which is twice-removed (imitative in the bad sense). But Plotinus' view that 'the arts create much from themselves'

<sup>1</sup> *Op. cit.*, p. 86.

<sup>2</sup> P. 238.

<sup>3</sup> See e.g. T. Whittaker, *The Neo-Platonists*, pp. 54 sqq., 264 sqq.

is simply the Platonic attitude towards genuine art. And it does not necessarily mean that the art of Homer is imitative in any other sense than the sense in which Plato regarded it as imitative. Proclus too agrees with Plato in principle; in applying his principles he differs from Plato simply because his respect for Homer (which he does not attempt to justify rationally) impairs his deductive powers. Since such writers do not impugn Plato's principles, let us put our question in a more definite form; let us ask not what is true about poetry or art in general or in the abstract, but what is true of this particular and concrete poetry, that, say, of Homer. Were there any of these later writers who argued seriously against Plato that inspiration of the Homeric variety meant knowledge, that Homer was a man of knowledge, not of mere opinion, and that therefore his works were imitative in the good sense and not in the bad?

I can find no trace of any. A typical instance is Strabo.<sup>1</sup> Strabo thought Homer a very wise philosopher. But by philosophy he meant not a way of life or a science of first principles but the possession of multifarious information. Homer was a philosopher because he knew a great many useful facts, for example, about geography; and it was by conveying this kind of knowledge that he aimed at the moral improvement of his readers. Strabo's only argument against Plato is to the effect that one who is ignorant of life could not imitate it so well as Homer did; since, then, Homer imitated life so well, he must have been a very wise man. This is not a serious criticism for Plato to meet. But where did Strabo get it? Evidently from the same source from which come so many criticisms of Plato, namely, Plato himself. Strabo was drawing upon *Rep.* 598e, where this argument of his is not merely put but answered. The fact that Strabo draws on Plato for an 'answer' to Plato's attack on Homeric imitation is a pretty good indication that the Stoics had left this subject severely alone. Proclus is also typical. He tries to commit Plato to an esotericism (in the matter of the interpretation of the myths of the poets) of which Plato obviously disapproved. He repeats that Homer was 'inspired'; but he has no answer to Plato's attack on the inspiration which is divorced from knowledge. He does not attempt to prove that Homer's inspiration was of the other kind, which consists in the vision of the ideas. It is clear that, as I have already written elsewhere, these later writers made no serious attempt to deal argumentatively with Plato's attack on Homer.<sup>2</sup> They do not seem even to have thought of the answer that not reason but opinion, feeling and imagination are the true instruments of knowledge. For that view to be seriously advanced in mistaken defence of poetry the world had to wait till Plato had been temporarily eclipsed by Kant, and the romantic movement broke loose upon literature.

We find also in Proclus the remark, still common nowadays (it seems to Mr. Sikes<sup>3</sup> successfully to 'turn the tables on Plato'), that Plato and his dialogues would have been expelled from the ideal state because they are as highly imitative as Homer. This may be a neat enough gibe, though it ignores the two senses of the word 'imitative'; but it has no argumentative value. In the first place Plato was not writing his dialogues in an ideal state or to be read by the citizens of such a state. In the second place it is not so clear, as is generally assumed, that Plato would have been expelled from his own republic for imitating Thrasymachus and suchlike persons. When Plato in the *Republic*, *Phaedrus* and *Laws* laid down principles about writing, it need not be thought that he entirely exempted his own writings from those principles. What, then, are his dialogues meant to be according to his own account of the matter? Not text-books of philosophy (for philosophy cannot be learned from text-books), but memoranda and a form of amusement.<sup>4</sup> But is not all amusement forbidden to the Guardians? By no means. They are allowed to imitate (i.e.,

<sup>1</sup> I. 2, 2 sqq.

<sup>2</sup> *C.Q.*, January, 1930, p. 4.

<sup>3</sup> P. 238.

<sup>4</sup> *Phaedr.* 276d.

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<sup>1</sup> See m

<sup>2</sup> P. 75.

<sup>3</sup> *Ibid.*

ment with

to represent dramatically) baser men than themselves occasionally 'for the sake of amusement.' Let us then allow to Plato the same license as he gave to his Guardians. As I understand *Rep.* 369,<sup>1</sup> Mr. Sikes is too sweeping in his statement that Plato's 'Guardians must imitate only the virtuous.'<sup>2</sup>

This reference to *Rep.* 396 raises another question. Is it correct to say with Mr. Sikes that the limitations laid down in *Rep.* 392-8 'exclude both tragedy and comedy'?<sup>3</sup> Or is such an exclusion necessarily implied by the statement in 607a that only hymns to the gods and eulogies on good men will be admitted into the ideal state? I see no reason why these hymns and eulogies should not contain those forms of dramatic representation which are allowed to the ideal poet in 396. Such a poet may imitate good men like himself. There is nothing to prevent him from writing a dialogue between two good men as a part of his eulogy or hymn; and that would be drama of a sort. He may also on occasion impersonate even baser men, as I have mentioned above; there, surely, is a chance for the introduction of a comic element. (Nor, by the way, is it true that Plato required epic verse to be altogether 'shorn of the speeches.') Let us suppose, for example, a choral hymn of some length, interspersed with a good deal of direct narration and some dialogue, the whole making an organic unity in accordance with the doctrine of the *Phaedrus*. Is there any reason for refusing to such a piece the name of tragedy? At any rate, to say that Plato refused to allow tragedy and comedy means that he excluded all dramatic representation; which amounts to ignoring what Plato himself wrote in *Rep.* 396. And it is clear that Plato did not intend to exclude what he meant by tragedy; compare that passage of the *Laws* in which he confesses to being an imitator (in the good sense), and not merely an imitator but a tragic poet, for in depicting the lineaments of the ideal state he is making an imitation (*Laws* 817b, *μίμησις*) of the best and noblest life, and genuine tragedy is, he says, essentially the same thing as this. (It is obvious from all this that Plato did not need to be told by anyone that his dialogues were imitative; but unlike his critics he was aware of the fact that he used the word in more than one sense.)

Considerably less than justice is as a rule done to the poetry which Plato would admit into his ideal state. Professor Saintsbury,<sup>4</sup> for example, wrote that Plato's ideal poet was 'at the very best' 'a sort of Board-Schoolmaster.' Evidently he shared the common view that the poetry of the ideal state would be uninspired, unpleasing, mechanical and full of trite moralization—something like most of the formal odes of poets laureate. I hope to deal elsewhere with Plato's attitude towards the moralizing or didactic view of poetry. Here I wish merely to point out (without sharing Saintsbury's evidently low regard for the profession of pedagogue) that Platonic poetry is not meant to be a text-book of information (for the Platonic theory of education discounts the text-book), nor is his ideal poet meant to be a pedant but a man of genius<sup>5</sup>—a philosopher and an enthusiast whose influence would be good for the old as well as the young. Even his second-best poet (in the *Laws*) retains his spontaneity and his natural gifts, and is merely subjected to restrictions in matters which do not affect the essence of poetry. Such criticisms are founded on an erroneous notion of what philosophy is or was meant by Plato to be. They assume that philosophy is something merely dry and abstruse and entirely remote from life, or at least from all that makes life worth living. They assume that morality is an essentially uninteresting business, and that Plato's ideal realm is (as perhaps it is—but Plato did not think so) a colourless blank. But for Plato philosophy meant a life to be lived. Its prin-

<sup>1</sup> See my analysis in *C.Q.*, 1928, p. 18.

<sup>2</sup> P. 75.

<sup>3</sup> *Ibid.* I should like to add that my disagreement with Mr. Sikes on the points indicated

does not mean that I deny the obvious merits of his book.

<sup>4</sup> *History of Criticism*, I., p. 19.

<sup>5</sup> *Rep.* 401c. Cf. *Phaedr.* 269-70.



ciples were meant to be not restrictive but emancipating, not barren but fruitful. They were not to be mere rules imposed by an external authority but internal principles informing the whole personality of the Platonic citizens, who were to appropriate them either (at best) by thinking them out for themselves or (at least) by putting them into practice. The philosopher-poet of the *Republic* was to resemble the ideal world in his own character as well as to reflect it in his writings.<sup>1</sup> But such a requirement seemed to Saintsbury 'monstrous.'

The difficulty in dealing with such criticisms lies in the fact that their authors have not thought out, or at least have not troubled to express clearly, the metaphysical implications inherent in their critical standpoint. When Saintsbury writes that the Platonic theory of imitation conjoined with the theory of ideas 'prevents Plato from seeing that the poet's duty . . . is to *disrealize*, to give us things not as they are but as they are not,' I must confess that I find the philosophy which lies behind this criticism a more nearly 'unintelligible caprice' than any of the absurd whimsicalities which Saintsbury apparently found in the *Republic* and the *Laws*.<sup>2</sup> I find it easier to understand the Platonic view that the poet's function is to make his poetry an imitation or reflection of the ideal world which for Plato is the truly real world. This does not mean that according to Plato the function of art is to impart knowledge. Mr. Sikes<sup>3</sup> asks why Plato failed 'to make it plainer that Art . . . might be at least a valuable instrument of knowledge.' The answer is that according to Plato art cannot impart *knowledge* (*Rep.* 522a), though it may infect both young and old with harmoniousness of character and (in the case of poetry at least) familiarize the young with right opinions. The ideal poet is inspired by the vision of the ideas; his work is produced in the light of ideal knowledge (402bc). Hence it is both true and beautiful; but it is not an instrument of knowledge, which is won not by believing what one reads but by testing it and understanding it by oral question and answer.<sup>4</sup>

The works of the philosopher-poet who, thanks to a natural gift and the vision of the ideas, can produce poetry which is imitative in the good sense of that word will naturally have an ethically good effect. Plato is often blamed, though with no good reason, for dwelling on the ethical aspect of his doctrine. It is clear that imitation (even of the merely external variety) tends to produce or encourage in the imitator the qualities which he imitates. The poet and his readers identify themselves sympathetically with the heroes of poetry, and so tend to become really like them. There was no reason why Plato should blind himself to this obvious fact and its ethical implications. There was indeed a very good reason why he should not ignore the ethical problem. It was not he who raised it. It had long been the 'universal opinion' that youths should be saturated with the works of the poets, and the practice was justified (as by Protagoras in the dialogue named after him) by the theory that boys who had learned from the poets rules of conduct and eulogies of famous men would in later life obey those precepts and imitate those examples. Niceratus, for example, according to Xenophon, had been caused by his father Nicias to learn by heart the poems of Homer in order that he might grow up a good man, as brave as Achilles and as wise as Nestor.<sup>5</sup> Whether or not such a course of education turned out Homeric heroes, it certainly produced extreme partisans of Homer of the type attacked by Plato in *Rep.* X. Since these adherents of Homer praised their idol largely on ethical grounds, it was necessary for Plato, when dealing with ethics and education, to criticize their views. Plato, as a teacher of ethics, was really, in the circumstances of the time, competing with Homer. His Homeric criticism is part of his attempt to save his contemporaries from preferring the Homeric ethics to the Platonic. Homeric ethics may be of no interest to the modern literary critic. They

<sup>1</sup> *C.Q.*, 1928, p. 19.

<sup>2</sup> *Op. cit.*, pp. 18-19.

<sup>3</sup> *Op. cit.*, p. 86. <sup>4</sup> *Phaedr.* 277-8; *Prot.* 347.

<sup>5</sup> Cf. *Laws* 810-1; *Prot.* 326; *Xen. Symp.* III. 5, IV. 6-7; Girard, *L'Éducation Athénienne*, pp.

151 sqq.



exist for all that; and they were of considerable importance for many Athenians of the fifth and fourth centuries.

Plato, then, took up the ethical problem of the imitation of poetic characters from others who had already dealt with it incorrectly. He accepted the common belief that the poets set before us examples to imitate, and at the same time deepened and corrected that belief, holding that poetry was not to incite the reader to be like such models as Achilles and Odysseus in every respect, but should help both reader and poet to be truly virtuous men. The distinction between intelligent and unintelligent imitation was thus of great ethical importance—especially as the Greeks seem to have been particularly prone to imitating their favourite poetic characters. Plutarch, centuries later, found it still necessary to advise that young readers of poetry should be warned against regarding heroes like Achilles as paragons of virtue to be scrupulously imitated in one's own conduct. Their good and evil traits must, he says, be distinguished, and the former alone emulated. Plutarch writes as a Platonist who has to put up with non-Platonic poetry; if poetry cannot be found which is intelligently and Platonically written, then such poetry as is available must be intelligently and Platonically read. He lays down rules for the reader and not (like Plato) for the poet; for the question for him is what kind of imitation the reader should permit himself to practise. I should like to quote here the very apt example which he uses to illustrate the two kinds of imitation: it would be an unintelligent way of attempting to become a genuine philosopher if one set out to imitate Plato's stoop or Aristotle's stammer. On the other hand, the intelligent imitation of them—i.e., the imitation not of mere externals but of the ideas expressed in their thought and lives—would involve making their ideals one's own and tend in some degree to bring about their actualization in one's self.

Let me say in conclusion that I am not defending Plato's treatment of poetry as though it represented my own views. I am defending it against certain criticisms which appear to me premature because they are not based on a correct statement of Plato's attitude. Unless Plato's views are correctly and clearly stated it is of little use to attack them. (My point is that Plato's treatment of poetry is in accord with his general metaphysical position. If I do not accept the former (as is in fact the case) it is in so far as I reject the latter. I suggest that literary critics who are anxious to overturn Plato's views on poetry should begin with a reasoned criticism of his metaphysics, or at least with a reasoned statement of their own. There is no point in dragging Plato before the bar of 'opinion'; he would refuse, and rightly, to recognize the court.)

J. TATE.

THE UNIVERSITY, ST. ANDREWS.

## SULLA'S NEW SENATORS IN 81 B.C.

ONE of Sulla's first acts on assuming the dictatorship in 81 B.C. was to fill up the numbers of the Senate by the addition of some 300 new members. Tradition is divided on the question of the rank of these men before their promotion, and no unanimity has yet been reached in the matter. There are two distinct versions in the ancient authorities, both equally well attested. Appian<sup>1</sup> and the Epitomator of Livy<sup>2</sup> state that the new members were *equites*, while Sallust<sup>3</sup> and Dionysius of Halicarnassus<sup>4</sup> assert that they included men of the lowest rank (*gregarii milites* is Sallust's phrase). When we turn to modern historians we find opinion similarly divided. Writers such as Lange,<sup>5</sup> Zumpt<sup>6</sup> and Herzog<sup>7</sup> accept the first version, whereas Niebuhr,<sup>8</sup> Botsford,<sup>9</sup> Willems<sup>10</sup> and Gelzer<sup>11</sup> incline to the view of Sallust. It is tempting to do as some writers have done and to combine the two versions, saying that some of the new senators were *equites* and some common soldiers.<sup>12</sup> But this is a very arbitrary cutting of the knot. Herzog seems to play with the idea that Sulla promoted ordinary soldiers via the *equites* when he says 'wobei noch dazu mancher war, der seine Ritterstellung erst eben errungen hatte.' This, however, is mere guesswork.

The balance of modern opinion seems to favour Sallust's version, probably because he was a contemporary authority. But we must always be on our guard in dealing with Sallust's statements about the Senate, and it is very significant that Appian, who usually follows Sallust,<sup>13</sup> has here deserted him, and preferred Livy or some authority whom Livy used. Appian was himself an *eques* and specially interested in this period, so that his disagreement with Sallust is important. Dionysius' support of Sallust adds no weight to his statement.

Apart from the mere testimony of Sallust, two other arguments have been used in favour of his view. The first is that centurions were among those whom Sulla promoted.<sup>14</sup> This idea appears to come from Tacitus' reference to the senator Ateius Capito, whom he describes as *avo centurione Sullano, patre praetorio*.<sup>15</sup> But Tacitus does not say that Capito's grandfather was ever a senator, and the fact that he says nothing about any promotion is almost conclusive evidence that the grandfather remained a centurion. The Latin implies that Capito's senatorial rank came from his father (quaestor in 55 B.C.), who no doubt won promotion in the ordinary way. Only one case is known to me of a man who was a centurion before Sulla's dictatorship and was later a senator. This is L. Fufidius, described by Orosius as a *primipilaris*,<sup>16</sup> who gained access to the Senate by currying favour with Sulla, and later became governor of Baetica.<sup>17</sup> But he was not promoted straight into the Senate, but was made to sue for office in the ordinary way before being admitted.<sup>18</sup>

The other argument in favour of Sallust's version is produced by Willems, who

<sup>1</sup> *Civ. I.* 100.

<sup>2</sup> *Epit.* 89.

<sup>3</sup> *Röm. Alt. III.* 155-6.

<sup>4</sup> *Criminalrecht. II.* 295 sq.

<sup>5</sup> *Gesch. und Syst. I.* 513.

<sup>6</sup> *Lectures II.* 388.

<sup>7</sup> *Roman Assemblies*, p. 418.

<sup>8</sup> *Le Sénat I.* 407.

<sup>9</sup> *Die Nobilität*, etc., p. 3.

<sup>10</sup> E.g. Pauly-Wiss. s.v. *Cornelius Sulla*; Rice-

<sup>11</sup> *Ant. Rom. V.* 77.

<sup>12</sup> *Ant. Rom. V.* 77.

Holmes, *Roman Republic I.*, p. 62.

<sup>13</sup> Rosenberg, *Einleit. und Quellenkunde*, p. 206.

On Appian's use of Livy see *Year's Work*, 1925-6, p. 37.

<sup>14</sup> Pauly-Wiss. *l.c.*; Gelzer, *op. cit.*, p. 3.

<sup>15</sup> *Ann. III.* 75.

<sup>16</sup> *V. 21.* 3, and cp. Pauly-Wiss. s.v.

<sup>17</sup> Sall. *Orat. Lepidi* 22; Plut. *Sull.* 31. 3; id.

*Sert.* 12, etc.

<sup>18</sup> Sall. *l.c.*

points to the alleged degeneracy of senators after Sulla's régime.<sup>1</sup> It is very dangerous indeed to generalize on such a subject as this, and particularly dangerous to use as evidence the *Pro Cluentio* and the *Verrines*, where Cicero is attacking the senatorial juries. Against the picture in these speeches we must set Cicero's statement about the same body in the *Pro Sulla*: *uniuersi senatus communis est laus ut constet post hominum memoriam nunquam in illo ordine plus uirtutis, plus amoris in rempublicam, plus grauitatis fuisse*.<sup>2</sup> There is, in fact, no more reliable evidence for the deterioration of the Senate after Sulla than is provided for the preceding period by Sallust's *Jugurtha*, a notoriously biased document. Apart from this, however, Willems' whole argument is unsound. He quotes altogether thirteen senators as evidence for his view, but it is not possible, as we shall see, to be certain that any given individual was a nominee of Sulla. Willems merely assumes that because these thirteen men were corrupt they were senators promoted by Sulla, and from this he argues that Sulla promoted corrupt men to the Senate, an obvious *petitio principii*. Of these thirteen, one, Staienus, is now said to have entered the Senate by the ordinary channels in 77 B.C.<sup>3</sup> But even if we agree that the other twelve were possibly Sullan nominees, they do not support Sallust's statement, since they include an Antonius, a Cornelius Lentulus, a Curius, an Egnatius, an Aquillius and a Herennius, none of whom can be justly described as *gregarius miles*. The story that Sulla introduced nobodies into the Senate thus rests solely on the biased evidence of Sallust. It seems, moreover, hardly consistent with Sulla's general policy. It is almost universally agreed that he intended the Senate to be the governing body of the state. If so he would hardly allow it to be polluted by the addition of unworthy members.

The other version of these additions, that of Appian and Livy's Epitome, is that the new members came from among the *equites*. Those modern writers who accept this view all assume that the *equites* there mentioned were the middle-class business men first made a power in politics by C. Gracchus. Carried to its logical conclusion, this idea would mean, as Lange points out,<sup>4</sup> that Sulla was heir to the policy of C. Gracchus—surely an untenable point of view. Sulla's bitter hatred of this class needs no illustration here; and it is most improbable that he would give them what amounted almost to a majority in his new Senate, even if some of them had supported him in the Civil War. We must not forget that the Equestrian Order was twofold. Within the large class of equestrian business men was a smaller and much more exclusive group of *equites* who formed the Eighteen Centuries of *equites equo publico*. This twofold grouping was a relic of the earlier arrangement of the cavalry, and there was a great difference between the two groups. Membership of the Eighteen Centuries was a jealously guarded privilege, and was partially hereditary. These centuries had ceased to act as cavalry, and were now, in Livy's phrase, *seminarium senatus*, i.e. composed mainly of the sons and relatives of senators who were awaiting membership of the Senate. They served as officers in the army as a preliminary to entering on a senatorial career, and since the *plebiscitum* which forbade a senator to retain his *equus publicus*<sup>5</sup> they were all non-senators. Their numbers are not known precisely, but they probably exceeded 2,000.<sup>6</sup> Now as there were normally about one hundred places available in the Senate every ten years, and the average expectation of life of a man of senatorial age (30) was about thirty years,<sup>7</sup> it is obvious that at least four-fifths of the *equites equo publico* never became senators at all under ordinary circumstances. There was thus always a large surplus of young nobles waiting for admission to the Senate. I suggest that these were the men from among whom Sulla chose his new senators, and in support of this I would first draw attention to the terminology of Appian and the Epitome of Livy in the passages concerned.

<sup>1</sup> *Le Sénat* I., p. 411 sq.

<sup>2</sup> *Pro Sull.* 82.

<sup>4</sup> *Röm. Alt.* III., p. 156.

<sup>3</sup> Pauly-Wiss. s.v.

<sup>5</sup> *Cic. Rep.* IV. 2. 2.

<sup>6</sup> Cato ap. Priscian VII. 8, p. 317. κ.

<sup>7</sup> Willems, *Le Sénat* I., p. 161 sq.

Appian, as has been said, was himself an *eques*, and in the Imperial period that meant that he had the public horse. He frequently mentions the *equites*, and his terminology clearly shows that in speaking of Republican conditions he was aware of the existence of the two groups mentioned above, and distinguished carefully between them. The whole equestrian middle class he calls οἱ καλούμενοι ἱππεῖς, thus registering his opinion that nobody who did not possess the *equus publicus* was worthy the name ἱππεύς. In one passage he defines οἱ καλούμενοι ἱππεῖς as οἱ τὴν ἀξίωσιν εἰσι τῆς βουλῆς καὶ τῶν δημοτῶν ἐν μέσῳ.<sup>1</sup> The title ἱππεύς is reserved for the true *equites*, that is, according to Appian, those possessing the public horse—the Eighteen Centuries. Thus, Pompey, for instance, is described as ἔτι ὢν τῶν ἱππέων;<sup>2</sup> and in *Civ.* IV. 95 we are told that there were on the rostra ὑπάτων καὶ στρατηγῶν καὶ δημάρχων καὶ ἀγορὰ νόμων καὶ ἱππέων κεφαλαί. Here Appian mentions first magistrates in descending order of importance and then those who were preparing for the magistracy, as the next in rank to quaestors. If we apply this test to Appian's account of Sulla's measures, we find that the *equites* whom Sulla proscribed are called οἱ καλούμενοι ἱππεῖς,<sup>3</sup> but that those whom he promoted into the Senate are called οἱ ἀριστοὶ ἱππεῖς,<sup>4</sup> clearly showing that Appian believed the new senators to have been drawn from the Eighteen Centuries.<sup>5</sup>

The 89th Epitome of Livy says simply *senatum ex equestri ordine explevit*. If this were Livy's own expression it would give us the same meaning as Appian, since Livy always uses *equester ordo* for the Eighteen Centuries.<sup>6</sup> But the terminology of the Epitomes is not consistent. In most cases, e.g. in the reference to the reforms of C. Gracchus,<sup>7</sup> *equester ordo* means the equestrian middle class, but in at least one case it does not. This is in Epitome 23, which presents a close parallel to the one we are discussing. This relates to the filling up of the Senate by the dictator M. Fabius Buteo after Cannae, and its words are almost identical with those of Epitome 89—viz., *senatus ex equestri ordine suppletus est*. Luckily we can, in this case, see what *equester ordo* means by comparing the actual words of Livy in XXIII. 23. Here there is no mention of *equites* at all. Livy simply names three groups of men promoted to the Senate by Buteo: (1) *qui post L. Aemilium, C. Flaminius censors curulem magistratum cepissent, necdum in senatum lecti essent*; (2) *qui aediles, tribuni plebis quaestoresque fuerant*; (3) *qui magistratus (nondum)<sup>8</sup> cepissent, qui spolia ex hoste fixa domi haberent, aut civicam coronam acceperant*. Now Buteo declared his intention of departing as little as possible from the normal procedure in enrolling his new senators, so that the first two groups were simply ex-magistrates who were awaiting enrolment in the Senate by the next censors. The third group were military men of distinction who had the next claim for inclusion in the Senate. They can be no other than the members of the Eighteen Centuries who were preparing for public office.<sup>9</sup> All these

<sup>1</sup> *Civ.* I. 22.

<sup>2</sup> *Civ.* I. 80.

<sup>3</sup> *Civ.* I. 95.

<sup>4</sup> *Civ.* I. 100.

<sup>5</sup> Two points in connection with Appian's terminology are worthy of notice: (a) In long passages dealing with the equestrian middle class (e.g. *Civ.* I. 22) he introduces this class by the title οἱ καλούμενοι ἱππεῖς, but he does not repeat the καλούμενοι in subsequent references which are obviously to the same people. This shows that his use of καλούμενοι was a personal idiosyncrasy, and that he knew that once he had defined which *equites* he meant his readers would find ἱππεῖς sufficient.

(b) His reference to the proposals of the younger Drusus (*Civ.* I. 35) is striking. He says, τὴν τε βουλὴν καὶ τοὺς ἱππέας, οἱ μάλιστα δὴ τότε

ἀλλήλοις διὰ τὰ δικαστήρια διεφέροντο, ἐπὶ κοινῷ νόμῳ συναγαγεῖν ἐπειράτο. It is usually assumed that Drusus was concerned with the middle class, but Appian's terminology would imply the Eighteen Centuries. This raises a fundamental point about Drusus' policy which cannot be discussed here.

<sup>6</sup> See *Class. Phil.* XXV. 3 (July, 1930), pp. 244-9. Momms. *Staats.* III. 483, n. 3.

<sup>7</sup> *Epit.* 60.

<sup>8</sup> The reading here is doubtful. I agree that a negative is required, but I would suggest *nondum* rather than *non*.

<sup>9</sup> It is significant that the three groups here mentioned correspond to those mentioned by Appian IV. 95 (see above), where the last group



three groups would belong to the Eighteen Centuries, of course, since at this time men who had qualified for the Senate, and even senators, retained the *equus publicus*. These were the men whom Epitome 23 describes as *equester ordo*. We cannot be certain that the phrase means the same in Epitome 89, but the parallel is very close between them, since both refer to an extraordinary filling up of the Senate. Thus it is probable that Livy, like Appian, believed Sulla's new senators to have come from the Eighteen Centuries.

In ancient authorities, therefore, we have the word of Appian and Livy against that of Sallust. There is only one way in which we can decide between the two versions. That is by trying to discover actual cases of men promoted by Sulla and examining their previous rank. In an attempt to do this I have carefully examined the careers and antecedents of about 250 senators belonging to the period immediately following Sulla's dictatorship. From these I have selected eighty-one whose histories give grounds for the assumption that they were admitted to the Senate by Sulla. The principles on which this selection is based are as follows:

1. Probable Sullan nominees are senators about whom one or more of the following facts are known:

(a) That they held the praetorship, aedileship or consulship after 81 without having held the quaestorship. It can hardly be doubted that Sulla did not insist on all his new senators going through the full *cursus*, but allowed men who were old enough to stand for higher offices without holding the quaestorship.<sup>1</sup> Indeed to insist on everybody first holding the quaestorship would have ruined Sulla's plan for keeping up the numbers of the Senate, since men who were already senators would be preventing others from qualifying.

(b) That they reached the age of thirty during Sulla's absence from Rome (88-81 B.C.) and supported him in the Civil War.

(c) That they were already senators before being elected to the quaestorship.<sup>2</sup> This follows from the fact that after 81 the quaestorship was the key to the Senate, so that no ordinary member elected to that office after that date can have been a senator before.

(d) That though their age and career are not known, they fought for Sulla in the Civil War.

2. Any senator of whom any of the following is true cannot have been a nominee of Sulla:

(a) That he was under thirty years of age in 81. Sulla fixed thirty as the minimum senatorial age, and the case of Pompey shows that he applied this test to those admitted in 81.<sup>3</sup>

(b) That he was made quaestor after 81, unless he was previously a senator. [See 1 (c) above.]

(c) That he was a senator before 81. To this there are some possible exceptions, for which see p. 174.

A selection made within these limits must contain a fair proportion of men who were actually Sulla's nominees, though it is not possible to state this positively about any one individual. The senators in this selected list fall into certain groups which are set out below.<sup>4</sup>

are *ἱππεῖς*. They also correspond closely with the categories excluded from the juries by the so-called *Lex Acilia*, the last of which is usually taken to be the *equites equo publico* (see E. G. Hardy, *Roman Laws and Charters*, p. 14).

<sup>1</sup> E.g. L. Cornelius Sisenna and D. Iunius Brutus, p. 175.

<sup>2</sup> E.g. P. Sulpicius Galba, Cic. *Verr.* I. 10. 30,

and Scol. Gron. *ad loc.*, p. 395 (Orelli).

<sup>3</sup> Willems, *op. cit.* I., p. 409.

<sup>4</sup> The chief works used in this compilation have been Pauly-Wissowa and, where this is incomplete, Smith's *Dictionary of Biography*. In addition, valuable help was gained from Willems' *Le Sénat* and Orelli's *Onomasticon Tullianum*.

1. The first group consists of men who were senators before the Civil War, but lost their senatorial status owing to their support of Sulla. These would include most, if not all, of Sulla's senatorial officers, and of course, technically, Sulla himself. Some were actually exiled or outlawed by Cinna, while others fled to join Sulla in Asia and thus sacrificed their status. Such men would naturally have first claim for the vacant places which Sulla had to fill, but it is a moot point whether they are to be reckoned among the additions to the Senate, since theirs was merely a case of restoration. As to their rank, technically the loss of senatorial status meant reduction to that of *eques*, but many of these men would cease to have citizen rights at all. They cannot, therefore, be used as evidence for either of the ancient versions. As examples of this group we may quote Q. Lutatius Catulus, C. Aurelius Cotta and Appius Claudius Pulcher, all well-known men.<sup>1</sup> Catulus, who was quaestor before 87 B.C., was proscribed by Cinna and escaped to Sulla. Cotta, the famous orator, who, from the fact that he stood for the tribunate in 91 B.C., may be assumed to have been a senator at that time,<sup>2</sup> was exiled under the Varian law, and did not come back to Rome until Sulla's return. Appius Claudius, the father of P. Clodius and praetor in 89 B.C., was passed over by the censor of 86, L. Marcius Philippus, and thus deprived of his senatorial rank. He too fled to Sulla and returned with him in 82. All these three men were restored to the Senate by Sulla, since they held office, Catulus as consul in 78, Cotta as consul in 75, and Appius Claudius as consul in 79. I have noted eleven others of the same type,<sup>3</sup> but there must have been many more.

2. We are on more certain ground in dealing with the next group. In addition to the senators mentioned above, Sulla was accompanied, in his voluntary exile, by a number of younger men of senatorial family who had not yet held office. These were the chief element in the Eighteen Centuries, and thus they are of vital importance to our argument. Being on the point of becoming senators before the Civil War, they would naturally, by joining Sulla, cut themselves off from their senatorial career for the time being, and they would be the first claimants (apart from the ex-senators) for inclusion in the reconstituted Senate. This group includes several famous names, and the most famous of all is Crassus the Triumvir. Born in 115 B.C., he reached senatorial age during the régime of Cinna, while in exile. He returned with Sulla in 82, and we find him shortly afterwards a member of the Senate, though not given a very prominent position by Sulla.<sup>4</sup> With Crassus Cicero compares the two Lentuli, Cn. Cornelius Lentulus Clodianus and P. Cornelius Lentulus Sura.<sup>5</sup> Their cases are exactly similar. Another interesting case is that of Hortensius, the orator. He was born in 114 B.C. of a senatorial family, and was therefore an *eques equo publico*. Throughout Cinna's reign of terror he remained a staunch optimiate and defended young Pompey in 85 against the charge of malversation. It is not known when he entered the Senate, but he was aedile in 75, and subsequently praetor and consul. When we consider his services to Sulla's party and his friendship with Pompey, along with the fact that he was just over senatorial age in 81, we are almost compelled to believe that he was promoted to the Senate by Sulla.<sup>6</sup> Q. Caecilius Metellus (Creticus), from the fact that he stood for the praetorship in 75, may be assumed to have begun his senatorial career about the time of Sulla's dictatorship, and the same may be true of his brother Marcus, praetor in 69, who was present at the trial of Roscius of Ameria.<sup>7</sup> Their family was strongly optimiate and had

<sup>1</sup> For all these three and the facts quoted about them see P.-W. s.vv.

<sup>2</sup> Appian, *Civ. I.* 37, appears to include him among the *ἐπιφανέστατοι τῶν βουλευτῶν* of 90 B.C.

<sup>3</sup> C. Annius, M. Aurelius Cotta, L. Bellienus, Cn. Cornelius Dolabella, L. Licinius Lucullus, L. Licinius Murena, L. Manlius, M. Pupius

Piso, C. Scribonius Curio, M. Servilius, P. Servilius Isauricus. For all these see P.-W. s.vv.

<sup>4</sup> Plut. *Crassus* 6. 7.

<sup>5</sup> Cic. *Brut.* 308. 311.

<sup>6</sup> P.-W. s.v. states that he entered the Senate in 78, but without evidence.

<sup>7</sup> See P.-W. s.vv. and Cic. *Rosc. Am.* 77. 119.

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rendered valuable assistance to Sulla, so that they were probably rewarded with a seat in the Senate. Two older men of whose careers nothing is known before 81 are L. Cornelius Sisenna, the historian, and D. Junius Brutus. Sisenna, born in 118 B.C., was a supporter of Sulla and belonged to the same *gens*. We find him praetor in 78 B.C.<sup>1</sup> Brutus was born in 120, fought for Sulla in the Civil War, and was consul in 77 B.C.—i.e. *suo anno*.<sup>2</sup>

It is tempting to include in this group M. Terentius Varro, the author. Cichorius<sup>3</sup> concludes that Varro was quaestor in 86 and that his next office was that of proquaestor in 76, but his evidence is very flimsy. Even if the detached fragment he quotes does relate to Varro himself, which is by no means certain, the expression *caballum reduxi ad censorem* is not necessarily equivalent to *equum reddidi*, as Cichorius supposes. It is more likely to be an echo of the phrase *equum traduxi*, and to mean simply that he returned to Rome and underwent the usual censorial inspection at the end of his *militia*. It is thus no proof that he gave up his public horse and became a senator on that occasion. If this happened in 86, as Cichorius convincingly argues, then Varro was still an *equus equo publico* in that year. His close connection with Pompey and his service as legatus or proquaestor in the Sertorian war<sup>4</sup> may justify the conjecture that he became a senator in 81.

In spite of the poverty of our records, quite a large number of these young senatorials can be traced. I have noted as resembling Crassus, in addition to those already named, thirty men, and these cannot be more than a portion of them.<sup>5</sup>

3. In his recent book on Roman politics, Münzer states that it was part of Sulla's policy to revive ancient aristocratic families.<sup>6</sup> I have noted some half-dozen cases of men apparently promoted by Sulla who seem to belong to families which had once been great but had suffered an eclipse. The most important is that of M. Domitius Calvinus, praetor in 80 B.C. and governor of Hither Spain in 80 and 79. No member of his family, as far as we know, had been in the Senate before him since Cn. Domitius Calvinus was curule aedile in 304 B.C.<sup>7</sup> The notorious Bulbus,<sup>8</sup> whose full name was probably M. Atilius Bulbus, belonged to a family not, apparently, represented in the Senate since 245 B.C., when an Atilius Bulbus was consul. C. Aquilius Gallus,<sup>9</sup> the great jurist, was not the first of his family to gain office, but between him and his nearest senatorial ancestor (L. Aquilius Gallus, praetor in 176) is a gap which may mean a decline in the family fortunes, which were revived by Sulla's promotion of Gaius to the Senate. A very interesting case is that of C. Licinius Sacerdos, praetor in 75 B.C., who, according to Asconius,<sup>10</sup> *tantum non primus ex familia sua magistratum adeptus est*. We are assured by the same writer that he was not, like Cicero, *equestri loco natus*. He was probably the grandson of the C. Licinius Sacerdos, a member of the Eighteen Centuries, whom Scipio as censor in 142 B.C. accused of perjury but would not prosecute for lack of other

<sup>1</sup> Teuffel I. 156.

<sup>2</sup> P.-W. s.v., No. 46.

<sup>3</sup> *Röm. Stud.*, p. 219 sq.

<sup>4</sup> *Id. ib.*, pp. 193-4.

<sup>5</sup> M'. Acilius Glabrio (P.-W., No. 38), M'. Aemilius Lepidus (P.-W., No. 62), M. Antonius Creticus (P.-W., No. 29), C. Antonius Hybrida (P.-W., No. 19), M'. Aquilius (Willems, *op. cit.*, I., p. 412), Cn. Aufidius Orestes (P.-W., No. 32), M. Baebius (P.-W., No. 18), C. Calpurnius Piso (P.-W., No. 63), L. Cassius Longinus (P.-W., No. 64), C. Claudius Glaber (P.-W., No. 165), C. Claudius Nero (P.-W., No. 247), C. Cornelius Lentulus Caudinus (Cic. *Pro Clu.* 107), P. Cornelius Sulla (P.-W., No. 386), Q. Cornificius (P.-W., No. 7), C. Fonteius (P.-W., No. 7), L. Furius (P.-W., No. 18), A. Gabinus (P.-W.,

No. 10), L. Lucretius Trio (P.-W., No. 33), A. Manlius (P.-W., No. 13), Q. Manlius (P.-W., No. 34), T. Manlius Torquatus (P.-W., No. 85), Q. Marcius Rex (P.-W., No. 92), Sex. Nonius Suffenas (Willems I., p. 450), Sex. Peducaeus (Smith, No. 2), Q. Petillius (Smith, No. 6; cp. Willems I., p. 504), C. Publicius Malleolus (Smith, No. 4), P. Sulpicius Galba (P.-W., No. 55), Terentius Varro (Willems, p. 451), P. Valerius Triarius (Willems I., p. 455), C. (or L.) Valerius Triarius (Smith, No. 1).

<sup>6</sup> *Röm. Adelsparteien*, p. 302 sq.

<sup>7</sup> P.-W. s.v., No. 44.

<sup>8</sup> Cic. *Pro Clu.* 26. 71.

<sup>9</sup> P.-W. s.v., No. 23; Plin. *N.H.* XVII. 1.

<sup>10</sup> *Tog. Cand.* 73.



testimony. We know nothing more of the grandfather and nothing at all of the father of our senator. We may therefore speculate that the stigma attaching to the grandfather prevented him or his son from rising out of the Eighteen Centuries, and that the family was restored to its place in the Senate by Sulla's promotion of the grandson. Men of this kind, being of senatorial descent, would almost certainly be members of the Eighteen Centuries, and would be promoted from there.<sup>1</sup>

4. All those mentioned up to this point have been quite clearly men of senatorial family. We come now to a fourth group of possible Sullan nominees who do not appear to have been of senatorial birth. Of these probably the best known are Pompey's famous lieutenants, L. Afranius<sup>2</sup> and M. Petreius.<sup>3</sup> Afranius, whom Cicero contemptuously calls *Auli filius*, presumably because of his obscure origin, served as *legatus* to Pompey in the war against Sertorius in 77 B.C. This implies that he was a senator, and therefore probably put into the Senate by Sulla as a favour to Pompey. Petreius had, in 63, when serving against Catiline, had thirty years' military experience, i.e. he began his military career about 93 B.C. This would make him of a suitable age to become a senator in 81, and it seems likely that he too owed his promotion to the patronage of Pompey, whom he later served so faithfully and well. Another similar case is that of C. Memmius, the patron of Lucretius and brother-in-law of Pompey. He served under Pompey in Sicily, apparently in some senior position,<sup>4</sup> and may therefore have been a Sullan senator, though his quaestorship did not come until 76.<sup>5</sup> Sulla himself no doubt had his own protégés, such as M. Minucius Basilus,<sup>6</sup> who was an officer of his in 86, and L. (or P.) Octavius Balbus,<sup>7</sup> leader of his cavalry at the Battle of the Colline Gate, both of whom were senators in 74 B.C. Other Sullan generals appear to have sponsored their officers in the same way. Aquinus was *legatus* of Metellus,<sup>8</sup> and Lucullus perhaps sponsored Cassius Barba,<sup>9</sup> Sornatius<sup>10</sup> and Voconius Naso.<sup>11</sup> Crassus had Pomptinus as his *legatus* in 73.<sup>12</sup> Some of these may, of course, have entered the Senate in the ordinary way after 81, but there is little doubt that some were promoted at the request of their powerful patrons. There must have been numerous promotions of the same kind of officers from Sulla's various armies,<sup>13</sup> as Sulla naturally wanted a strong Senate of men who had served him well in the past. These men, too, belonged to the Eighteen Centuries.

The promotion of men of this type will probably explain Sallust's use of the phrase *gregarii milites*. Being officers, they could be described as *milites*, and the addition of *gregarii* is an example of a kind of misrepresentation used by politicians in all ages.

5. Finally there is a group of men who, before entering the Senate, were in business, or whose families are described as equestrian. These might be thought to support the view that some, at least, of Sulla's new senators belonged to the Equestrian Order outside the Eighteen Centuries. But this is not so. Members of the Eighteen Centuries regularly engaged in trade or in tax-farming,<sup>14</sup> and there is evidence that at any rate some of these men did belong to the Eighteen Centuries, while there is nothing to show that any one of them did not. T. Aufidius, we are told, was once a *publicanus* in Macedonia and later became governor of that province and of Asia.<sup>15</sup> The date of his governorship and his age (he died at a good old age in

<sup>1</sup> P.-W. s.v., No. 154, and cp. C. Furnius (P.-W. s.v., No. 3) and L. Racilius (P.-W. s.v., No. 1).

<sup>2</sup> P.-W. s.v., No. 6.

<sup>3</sup> Smith, *Dict. Biog.* s.v., No. 2.

<sup>4</sup> Plut. *Pomp.* 11.

<sup>5</sup> Oros. V. 23. 12.

<sup>6</sup> Smith, s.v., *Basilus* 1 and 2.

<sup>7</sup> *Id.* s.v., *Balbus*.

<sup>8</sup> P.-W. s.v., No. 1.

<sup>9</sup> Willems I., p. 506.

<sup>11</sup> Smith, s.v., *Naso*.

<sup>10</sup> P.-W. s.v.

<sup>12</sup> Smith, s.v.

<sup>13</sup> See e.g. Sextilius (P.-W. s.v., Nos. 2 and 3 [probably identical]), Q. Gallius (P.-W. s.v., No. 6), P. Varinius Glaber (Smith, s.v., *Glaber*).

<sup>14</sup> Livy XLIII. 16; Val. Max. VI. 9. 8; Pseudo-Ascon., p. 212.

<sup>15</sup> Val. Max. VI. 9. 7.

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<sup>1</sup> Cic.

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<sup>4</sup> Cic.

<sup>5</sup> *Id.*

<sup>6</sup> Cic.

<sup>7</sup> Cic.



46 B.C.<sup>1</sup>) make him a possible nominee of Sulla. But his family was senatorial, and therefore he was probably an *equus equo publico*. Q. Considius, capitalist and later senator,<sup>2</sup> is a similar case, as is C. Falcidius.<sup>3</sup> Calidius<sup>4</sup> was the son of an *equus Romanus* who seems to have been a business man, but the title *splendidus* applied to the father may mean that he had the public horse, and other Calidii were magistrates. The doubtful cases, such as P. Tadius,<sup>5</sup> M. Caesius,<sup>6</sup> Q. Titinius (brother of an *equus*)<sup>7</sup> and M. Seius,<sup>8</sup> may equally well have had the public horse. A more difficult case is that of P. Popillius.<sup>9</sup> He was the son of a freedman, who was expelled from the Senate by one of the censors of 70 B.C. Willems<sup>10</sup> assumes that he was a Sullan nominee because of his humble origin, though we have no other evidence that he was. Even if he was, however, there was nothing to prevent his being a member of the Eighteen Centuries.

There is nothing, therefore, in the antecedents of this final group to differentiate them from the four previous groups or to contradict what the terminology of Appian and Livy implies—i.e. that Sulla chose his new senators from the Eighteen Centuries of *equites equo publico*. In no single case, so far as I can discover, is there any kind of evidence to support the view expressed by Sallust and so generally adopted.

If we conceive of Sulla's additions to the Senate being made on this principle, we must regard him, in this respect as in others, as a true conservative and follower of tradition. There were two previous occasions in the history of Rome when it was necessary to fill up the numbers of the Senate by an extraordinary procedure, once in the first year of the Republic and again after the disaster at Cannae. On the former occasion, Livy tells us,<sup>11</sup> the first consul filled up the Senate with *primoribus equestris gradus*, who at that early date were undoubtedly members of the Eighteen Centuries. After Cannae, M. Fabius Buteo was made dictator for the purpose of filling up the Senate.<sup>12</sup> We have already discussed the method he adopted and come to the conclusion that his new members, too, were *equites equo publico*.<sup>13</sup> Thus Sulla followed the two available precedents, as a good conservative should. By his *Lex de Viginti Quaestoribus* he attempted to perpetuate this method of recruiting the Senate and to ensure that the censors should not be able to interfere with it.

NOTE.—With regard to Sulla's submission of his chosen senators to the vote of the *Comitia Tributa*,<sup>14</sup> I see no reason to reject the usual view that this was simply a matter of their confirming Sulla's selection. Hardy's objection to this view on the ground of Sulla's hatred of the Equestrian Order is removed if the *equites* Sulla promoted belonged, as I conclude, to the Eighteen Centuries.<sup>15</sup>

H. HILL.

UNIVERSITY COLLEGE, SWANSEA.

<sup>1</sup> Cic. *Brut.* 179.

<sup>2</sup> Val. Max. IX. 1. 1; Cic. *Pro Clu.* 107.

<sup>3</sup> P.-W. s.v.

<sup>4</sup> Cic. *Verr.* IV. 20. 42.

<sup>5</sup> *Id. ib.* I. 100; Smith s.v., No. 3.

<sup>6</sup> Cic. *Verr.* I. 130; P.-W. s.v., No. 9.

<sup>7</sup> Cic. *Verr.* I. 128.

<sup>8</sup> Cic. *Pro Planc.* 5. 12; P.-W. s.v.

<sup>9</sup> Cic. *Pro Clu.* 98. 131-2.

<sup>10</sup> I., p. 414.

<sup>11</sup> II. 1. 10.

<sup>12</sup> Livy XXIII. 23.

<sup>13</sup> See above, p. 172.

<sup>14</sup> App. *Civ.* I. 100.

<sup>15</sup> *J.R.S.* VI. (1916); I., p. 59 sq.

## ILIUPERSIDES.

For about a hundred years there has been an intermittent but sometimes vigorous debate<sup>1</sup> on the question whether Quintus Smyrnaeus and Tryphiodorus directly used the Second *Aeneid* as a source for their epic descriptions of the capture and destruction of Troy. Heyne<sup>2</sup> thought that they did not; but towards the end of the nineteenth century it appeared more likely that they did. Heinze<sup>3</sup> opposed the general belief: but it was reaffirmed for Quintus by Paschal and Becker<sup>4</sup> and for Tryphiodorus by Dr. E. Cesareo<sup>5</sup> on internal evidence. Meanwhile Professor Samuel E. Bassett<sup>6</sup> had concluded that Quintus was not after all dependent on Vergil: and still more recently Dr. J. W. Mackail, reviewing Cesareo's monograph,<sup>7</sup> has maintained with emphasis that the *Aeneid* is certainly not the source of the late epic. Accordingly the controversy cannot even now be considered closed.

I have no hesitation in raising the question again, partly because the present position is unsatisfactory, but still more because I believe that a new method can not only illuminate the unsolved problem of sources, but can also help towards a right understanding of Vergil's epic technique.

Hitherto in the controversy neither side has fully met the objections of the other. For example, Heinze rightly insists that the whole manner, spirit and *ethos* of the late poets are so different from Vergil's that Vergil cannot have been their source.<sup>8</sup> He notices that sometimes, when the late epic differs from the form of legend found in the *Aeneid*, it is the late epic, not the *Aeneid*, which represents an earlier stage of development.<sup>9</sup> But Heinze does not answer the argument that the verbal similarities are very numerous indeed and often exact. They are too many to be explained as coincidences,<sup>10</sup> or by reference to scattered parallels in Homer and in tragedy<sup>11</sup>; and mythological handbooks<sup>12</sup> cannot account for similarities of poetic phrases and thoughts. Cesareo, on the other hand, who is in the most direct—but always courteous<sup>13</sup>—opposition to Heinze, recognizes that Tryphiodorus persistently recalls

<sup>1</sup> For the references cf. especially R. Heinze, *Virgils epische Technik* (Leipzig, 1915), p. 63, note 3, cont. p. 64; P. Becker in *Rhein. Mus.* LXVIII. (1913), p. 68; E. Cesareo, *Trifiodoro e l'Iliupersides di Virgilio* (*Studi italiani di filologia classica*, N.S. VI. [Florence, 1929]), p. 234, note 1.

References to modern authorities will be given here by the name of the writer and page number only after the first mention of the work in question, unless there is a possible ambiguity. Initials are used as follows: V. for the *Aeneid* of Vergil: Q. for the *ῥὰ μὲν* 'Oμηρὸς of Quintus Smyrnaeus: and T. for the *Ἰλίου ἄλωσις* of Tryphiodorus.

<sup>2</sup> P. *Virgilius Maro varietate lectionum et perpetua annotatione illustratus* a C. G. Heyne (London, 1821), Vol. II., pp. 279: 284.

<sup>3</sup> Heinze, pp. 63 sqq.

<sup>4</sup> G. W. Paschal, *A Study of Quintus Smyrnaeus* (Diss. Chicago [1904]): Becker, pp. 68 sqq.

<sup>5</sup> Cesareo, pp. 231 sqq.: he carries much farther the method and conclusions of Dr. L. Castiglioni in *Riv. di fil. class.* N.S. IV. (1926), pp. 501 sqq.

<sup>6</sup> In *Amer. Journ. Phil.* XLVI. (1925), pp. 243 sqq.: endorsing the view of Koehly (*Quintus Smyrnaeus* ed. A. Koehly [Leipzig, 1850], *Prolegg.*, p. xxvi): cf. A. S. Way, *Quintus Smyrnaeus*, Loeb ed. (London, 1913), *Introd.*, pp. v. sqq.

<sup>7</sup> In *J.R.S.* XIX. (1929), pp. 107 sq.

<sup>8</sup> Heinze, pp. 63 sqq.: esp. pp. 66 sqq.: 69: 73: 81.

<sup>9</sup> Heinze, pp. 69: 72 sqq.: 76: 78: 81.

<sup>10</sup> Heinze, pp. 64 sqq.

<sup>11</sup> Heinze, p. 48, note 1, cont. p. 49: 50: 67, note 1: 77, note 1 (where Quintus appears nearer to Vergil than Sophocles to either). Cf. 77 sq. (on the Aeolus scene and storm, V. I. 50 sqq. ~ Q. XIV. 466 sqq.) for possible sources in Hellenistic poetry; and for parallels between Quintus and Sophocles, Bassett, p. 249 sq.

<sup>12</sup> Heinze, p. 67. Cf. C. Robert, *Bild und Lied* (Berlin, 1881), pp. 209: 222: 224: 229: cf. also pp. 208 and 231 and note 5, cont. p. 232, where the use of a handbook by Ovid is well established.

<sup>13</sup> Cf. especially Cesareo, p. 262, note 2. A like respect is due to Cesareo himself.

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Vergil in his words and phrases,<sup>1</sup> sometimes for line after line,<sup>2</sup> that his sequence of events is very like the Vergilian over long passages,<sup>3</sup> and that sometimes the similarities are found not in their Vergilian contexts but in others,<sup>4</sup> often in places where the Vergilian narrative is certainly not followed. An equally minute comparison of Quintus with Vergil would disclose conditions nearly but not quite the same. Yet however many parallels are found, and however exact they are, Heinze's argument for independence still stands. The dissimilarity is as decisive as the similarity. To answer this argument Cesareo has to assume that Tryphiodorus as a Greek or Hellenized Egyptian changed the whole orientation of Vergil's story from patriotic motives,<sup>5</sup> wishing to show the Achaeans in a better light. He must suppose that Tryphiodorus was anxious to follow Vergil closely but not slavishly,<sup>6</sup> and often to improve on him by expanding his expressions.<sup>7</sup> He is compelled to believe that Tryphiodorus paid so close an attention to Vergil that he often reproduced his phrases in a different context<sup>8</sup>; and to attribute to Tryphiodorus an unlikely ability to copy and blend together several originals at once.<sup>9</sup> If Quintus depends on Vergil, similar suppositions about his method are necessary also.

Cesareo fully works out his theory and faces its implications. But the implications are fatal, especially since the arguments of Heinze can be carried much farther than he carried them. These arguments are not purely subjective,<sup>10</sup> though Heinze tends to emphasize differences of spirit and poetic quality.<sup>11</sup> The late poets present their story in the form of Greek, not Latin, literature. If they had used Vergil, the Vergilian form of legend would have left more traces. But the late poets are often apparently quite ignorant of important Vergilian innovations. If they copied Vergil, they must have been able to copy his verbal expressions, and sometimes even their setting in his verse, without allowing his influence to affect the form of their story. They must also have had a surprising power of reconciling Vergilian inconsistencies: for they are on the whole consistent, but Vergil, in matters of legendary fact, is not. Their task must have been hard indeed: since to their other commitments they must have added the duty of working their way back from Vergil's narrative, often by careful disintegration and redistribution of Vergilian elements, until they reached a pre-Vergilian form of poetry and legend. Even the motive is lacking. Attention to Vergil cannot have helped the late poets to succeed in their plan, for their work is clearly not intended to be like Vergil's. Available Greek poetry must have been far more useful to them and less trouble to use. Besides, it is not known that these poets could even read Latin, and it is thought that this must be proved independently before a theory which assumes it can be accepted.<sup>12</sup> It is said that there is no evidence that the *Aeneid* was ever translated into Greek.<sup>13</sup> There is another argument from the verbal reminiscences themselves. They are by no means confined to the Second *Aeneid*.<sup>14</sup> If they had been, perhaps it would have been legitimate to use

<sup>1</sup> Cesareo, pp. 235 sqq.: *et pass.*

<sup>2</sup> Cesareo, pp. 245 sqq.: *et pass.*

<sup>3</sup> Cesareo, pp. 239 sqq.: 260: 277.

<sup>4</sup> Cesareo, pp. 240: 262: 274 sqq., especially 277. The displacement was noticed also by Castiglioni (p. 504: cf. 517).

<sup>5</sup> Cesareo, pp. 242: 264: 299.

<sup>6</sup> Cesareo, pp. 256: 270: 273.

<sup>7</sup> Cesareo, pp. 271: 286: 299.

<sup>8</sup> Cesareo, pp. 241: 274: 277: 286.

<sup>9</sup> Cesareo, pp. 268: 272. And yet Tryphiodorus himself says (3) that he writes in haste.

<sup>10</sup> As Becker (p. 90) argues.

<sup>11</sup> V., p. 178, note 8 *supr.*

<sup>12</sup> Mackail, p. 108. The argument of W. Kroll (*Flecheisens Jahrb. Supplementband*, XXVII. 2

[1902], p. 163) that the imitation of any Latin poet by any Greek is otherwise unknown should not of course be pressed. There is, however, definite evidence that Latin literature was very little known to distinguished Greek men of letters under the later Principate (Bassett, p. 243).

<sup>13</sup> Mackail, p. 108. But cf. Bassett, p. 243.

<sup>14</sup> Cf. e.g.: Q. I. 36 sqq.: 46: 53 ~ V. I. 490 sq.: 498 sqq.: XI. 659 sqq. (treatments of the motive of Penthesilea): Q. I. 826 sq.: VII. 672 sq.: XII. 104 sq. ~ V. II. 250: 268 sq.: IV. 522 sqq. (where 528 is rejected by Ribbeck: cf. V. IX. 225, which, however, fulfils the purpose of this comparison): XI. 182 sqq.: 201 sq. (treatments of the fall of night). With κατ' ὠκεανοῖο βεβήκει ἡδὺς . . . (Q. I. 826 sq.) should be compared especially *ruit oceano*



them as evidence that that book is a source of the late poetry. It is much less likely that the late poets would have included in their account of the sack of Troy, or indeed in any part of their poetry, reminiscences which they had drawn from many different parts of the *Aeneid*. That would need a very intimate acquaintance with Vergil and considerable Latin scholarship. It would be much easier to transcribe material from one poem on the sack of Troy in order to compose another. But the late poets, if they are proved by the reminiscences to have used Vergil as a source, must have done much more. Besides the reminiscences of words and details of thought, there are parallel incidents in different contexts<sup>1</sup>: and it is hard to believe that a late poet would have thought of using a Vergilian incident, from a very different place in legend, to form his own.

But whatever theory is adopted, the correspondences must be explained by it as well as the differences: and both are equally remarkable. One possibility remains: that the late poets used narrative poetry in Greek which Vergil also used. This possibility is obvious enough, but it receives little favour, partly because it is usual to suppose that the late poets employed mythological handbooks in prose,<sup>2</sup> to the exclusion of poetic sources, for the supply of their material. This supposition is not however necessary as a universal truth: and in the present instance the evidence is sufficient to refute it.

In discussions of the sources of the Second *Aeneid* and of the late epic the mysterious remark of a character of Macrobius is sometimes neglected<sup>3</sup>: that 'as

not (V. II. 250), because this may be one of the instances, of which others will be discussed below, of a difficulty in Vergil soluble by reference to the late epics. Possibly both phrases imply an ultimate original in which *ὠκεανός* was still located not only round but above the earth (as in Aesch. *P.V.* 128 sqq.: cf. *Et. Mag. s.v.*: J. E. Harrison, *Themis*<sup>2</sup> [Cambridge, 1927], pp. 456 sq.). There are, of course, objections to both the usual renderings of V. II. 250: a meaning 'down from the sky' agrees with Homeric precedent and with V. III. 508 and VI. 539. V. II. 8 sq.: 268 sq.: IV. 522 sqq.: IX. 222 sq.: XI. 182 sq.: 201 sq. may be *retractationes* (cf. A. M. Guillemin, *L'Originalité de Virgile* [Paris, 1931], pp. 125 sqq.) of a source of Q. XII. 104 sq. Cf. also: Q. XII. 261 ~ V. VI. 261: Q. XII. 365 sq. (of Sinon confronting the Trojans) ~ V. X. 693 sqq. (of Mezentius in battle): Q. XI. 405 sq. ~ V. XI. 283 sq.: Q. XII. 213: 400 sq. ~ V. X. 745 sq. (= XII. 309 sq.): Q. II. 605: XII. 459 sqq. ~ V. IV. 167 sq.: and parallels collected by Becker, *pass.* Cf. also: T. 116 (of Sinon: cf. also I 216) ~ V. I. 482 (of the Trojan Athena: cf. V. VI. 469 [of Dido]): T. 265 (of Sinon) ~ V. III. 613 (of Achaemenides). The incidents of Sinon and Achaemenides are linked by the occurrence of the same line as V. II. 76 and III. 612. Cf. Cesareo, pp. 278 sq. Probably Vergil used material concerning Sinon for the incident of Achaemenides. Cf. also: T. 330 sqq. (of Athena helping the wooden horse forward) ~ V. X. 246 sqq. (of Cymodocea helping the ship of Aeneas forward): T. 149 sq. ~ V. X. 280 sqq.: T. 310 ~ V. X. 501 sq.: T. 649 sq. ~ V. I. 39 sqq.

<sup>1</sup> Cf. e.g. the death of Dido on the pyre (V. IV. 504 sqq.: 665 sqq.) and the similar

death of Oenone (Q. X. 234 sqq.): the collapse of Dido under emotion (V. IV. 391 sq.) and the collapse of Cassandra (T. 439 sqq.), which seem all the more likely to be from a single original here concerned with Cassandra, because Ovid (*Her. V.* 199 sq.) uses to describe the plight of Cassandra words which recall Vergil's expressions about Dido: and such incidents as the tree-felling, for funeral rites in Italy (V. VI. 179 sqq.), and for building the wooden horse (Q. XII. 122 sqq.). The comparison of the departures from Carthage (V. V. 1 sqq.) and from Troy (Q. XIV. 370 sqq.) removes a difficulty in Vergil. The sailors of Aeneas, about to sail from Carthage, put garlands on their ships (V. IV. 418). Mr. John Sparrow (*Half-lines and Repetitions in Vergil* [Oxford, 1931], p. 97) argues that the references cited for garlanding ships are not true parallels, generally because they are not concerned with the start of a voyage. He suggests that V. IV. 418 is a *tibicen* inserted from *Georg. I.* 304, where it also occurs, but for the end of a voyage. But there is a parallel, which seems to have been missed, in Quintus, according to whom (XIV. 376) the Achaeans put garlands on their ships when they started on their return from Troy. It is therefore likely that here also Vergil and Quintus are both remembering a corresponding passage in earlier poetry. For the adoptions by Vergil of the form of an incident cf. R. S. Conway in *Martin Classical Lectures*, I. (1930: Cambridge, Mass., 1931), pp. 151 sqq.

<sup>2</sup> Cf. p. 178, note 12 *supr.*: and J. Conington, *P. Vergili Maronis Opera* (London, 1878), Vol. II., p. 108.

<sup>3</sup> *Sat. V.* 2. 5.

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every schoolboy knows' Vergil here followed Peisander almost verbally. That Peisander of Rhodes, whose date is about 650 B.C., wrote an *Iliu persis* is unlikely. The notices<sup>1</sup> seem only to refer to his poem on Herakles: but we are warned that much other poetry attributed to Peisander is spurious.<sup>2</sup> It is possible that Macrobius meant some other Peisander, perhaps otherwise unknown, and various views of the writer's identity have been maintained.<sup>3</sup> But little if anything can be proved, and it remains equally possible that Macrobius has wrongly reported the name of some Greek poet who wrote a narrative poem about the sack of Troy. Such mistakes are easy to make, even for competent writers. It seems much less likely that Macrobius has wrongly reported Sophocles, Euripides, or some other dramatic poet: and hardly credible that he would have expressed so emphatic a statement on an important question if there had been no ground at all for the opinion.<sup>4</sup>

The remark of Macrobius is therefore strong evidence that some single Greek narrative poem lies behind much of the Second *Aeneid*. If this is so it is not strange. The unknown Greek poem need not even have been Hellenistic. It is usually said that one of Vergil's important innovations was a return to early epic models from Hellenistic, which of course he did not therefore neglect altogether. Some Greek epic neither Homeric nor Hellenistic was read and highly valued at Rome in the time of Cicero.<sup>5</sup> Vergil preserves early but un-Homeric versions of legends.<sup>6</sup> Scholiasts explain passages of Vergil by reference to early hexameter poetry now lost,<sup>7</sup> which is therefore nearly certain to have been in the hands of Vergil himself. Much of this poetry probably survived for several centuries, long enough in fact to be used by the late epic poets. Even the cyclic epic seems to have survived; and it

<sup>1</sup> Collected by G. Kinkel, *Epicorum Graecorum Fragmenta*, pp. 248 sqq.

<sup>2</sup> Suid. s.v. *Πεισανδρος*.

<sup>3</sup> W. Schmid and O. Stählin, *Griechische Literaturgeschichte* (München, 1929), Vol. I. i., pp. 295 sqq., especially p. 297, note 9: Conington, Vol. II., pp. 107 sq.

<sup>4</sup> Arguments from the silence of other critics are not decisive: 'It is the wont of such witnesses to dwell rather on points of dissimilarity than on points of agreement' (Conington, p. 108). The implications of Serv. ad Verg. *Aen.* IV., etc., should not be exaggerated.

<sup>5</sup> Cicero thought highly of Antimachus (*Brut.* 51), and Statius used his *Thebaid* (Quintil. X. 1. 53). His date is early in the fifth century (Suid. s.v.: Plut. *Lys.* XVIII.). He related the return of Diomedes (Acron. ad Horat. *Epist.* ad Pis. 146), and Vergil may have used his work for his own references to Diomedes (*Aen.* X. 26 sqq., etc.). He certainly seems to have used the *Thebaid* of Antimachus in preference to Homer (A 402 sqq.) for Aegaeon: who in Vergil (*Aen.* X. 565 sqq.) and Antimachus (Interpr. Mai. ad Verg. *Aen.* X. 565) is the enemy, not, as in Homer, the friend, of Zeus. For the revived epic of Panyassis, Choerilus and Antimachus cf. Suid. s.v. *Πανύσσιος*.

<sup>6</sup> W. F. J. Knight in *Class. Phil.* XXV. (1930), pp. 358 sqq.: *id. ibid.* XXVI. (1931), pp. 472 sqq.

<sup>7</sup> Lost Hesiodic poetry is cited by Servius ad *Aen.* II. 82 (on Pelasgus), ad *Georg.* I. 14 (on Aristaeus), and ad *Georg.* III. 280 (on hippomanes). Cf. Hygin. *Fab.* 154, where apparently an otherwise unknown Hesiodic account of

Phaethon, a probable source of V. X. 185 sqq., is recorded. Cf. also the apparent influence on V. VII. 808 sq. (on Camilla) of Hesiodic lines (on Iphiclus), quoted by Eustathius ad *Il.*, p. 323, which are nearer to Vergil's words than T 227: and cf. too Schol. ad Arat. 322 (on Orion in Hesiod) with V. X. 763 sqq. The Hesiodic lines quoted by Schol. ad Pind. *Nem.* 16 (*Τηυέτην τ' ἐπέεσσα . . . διη τε Κελαινω . . .*) seem to have given to Vergil the association of 'Taygeta' (at the same place in the line) at *Georg.* II. 486 sqq. (where it raised difficulties in a controversy between Warde Fowler and R. W. Raper [*C.R.* XXVII. (1913), pp. 13 sqq. and 85], to which the solution perhaps lies in the Hesiodic influence and *imagination auditive*), and possibly to have suggested *dira Celaeno* at V. III. 211. (The probability is increased if *dirus*=*disus*=*dios* in some meaning like 'magically dangerous,' not 'bright,' as *dios* is still too often translated [J. E. Harrison, *Prolegomena to the Study of Greek Religion* (Cambridge, 1912), p. 23 and note 2, citing R. A. Neil in *J.H.S.* XIX. (1899), p. 114, note 1]. Cf. *dirus Ulixes* [V. II. 261] with *dios* 'Οδυσσεύς [a 196, etc.].)

That Vergil used early epic now lost is made likely by Servius ad *Aen.* XII. 691, citing from 'Homer' *σπρίζουσα λογχή* as a parallel to *striduntque hastilibus aurae*: by Dionys. Hal. *de Hom. poesi*, p. 294, citing from 'Homer' *φθέγγετο δ' ἥπλοχος νηὸς κνανοπρώποι* (a possible original of V. VI. 1, etc.): and by Probus ad *Georg.* II. 506, who says that Ennius and Vergil both follow 'Homer' in calling Tyre 'Sarra.' Extant Homeric poetry does not furnish these citations.

is apparently wrong to deny that Proclus had access to the text of it.<sup>1</sup> The Orphic *Argonautica*, written at about the same time as the poem of Quintus Smyrnaeus, implies a poetic source at least as early as the fifth century B.C.<sup>2</sup>

It is therefore likely that Quintus Smyrnaeus and Tryphiodorus used not the text of the *Aeneid* but Greek narrative poetry which had already been used by Vergil. This hypothesis explains equally similarities of thought and expression, and differences of legend and of spirit, provided that a satisfactory account can be rendered of changes imposed by Vergil on Greek material preserved in its characteristic quality by Quintus and Tryphiodorus. I propose, without further argument, to assume the theory to be true, and by examining a few passages of the three extant *Iliupersides* to investigate their relations to each other. If a coherent account of these relations can be given, I hope that three results will follow: that the theory of the sources of the three poems will be confirmed, that some difficulties and inconsistencies in the *Aeneid* will be explained, and that discoveries will be made about Vergil's artistic method. The comparison of the three texts seems to show that elements, often of small detail, which are used by Vergil are frequently found in the poetry of one or the other or both of the late poets, but usually in a different context or application. Yet normally it is in the late poets, not in Vergil, that the context or application of each element seems original. Often the transference or transmutation has left a difficulty unresolved in the existing draft of the *Aeneid*. This method of creating a new poetic complex from older elements seems primary in Vergil's art. I call it the Vergilian integration.

Sometimes Vergil's story seems to presuppose a foundation which appears in the late poetry but not in Vergil's. When the wooden horse was standing complete, the storm, which was supposed to have prevented the Achaeans from setting sail, became, according to Sinon in the *Aeneid*, more intense.<sup>3</sup> There is an impression that the construction of the horse had somehow released a new violence of the tempest,<sup>4</sup> which was clearly thought to be dominated by supernatural power. The relation is explicit in Quintus: who describes how there was a war among the gods about the horse, a war which was manifested to mankind as a storm.<sup>5</sup> Thus Quintus supplies what Vergil has in part suppressed, and seems to represent a tradition common to Vergil also.

The second of the two detachable passages which comprise the warning and death of Laocoon is begun by Vergil with words which appear to mean that 'a still more frightful thing' was about to occur.<sup>6</sup> The difficulty is that no other horrible portent, with which Laocoon's death might be compared, has happened.<sup>7</sup> The comparative expressions are not appropriate in Vergil: but in Quintus, who has them also at this context in verbal similarity,<sup>8</sup> they are justified. The Laocoon of

<sup>1</sup> T. W. Allen, *Homer, The Origins and Transmission* (Oxford, 1924), pp. 56 sqq. The quotations preserved, and the phrase used by Philo Bybl. (fr. 2. 28 Müller), seem to disprove the suggestion of Robert (p. 224) that the texts almost went out of circulation early.

<sup>2</sup> J. R. Bacon in *C.Q.* XXV. (1931), pp. 172 sqq.

<sup>3</sup> V. II. 112 sqq.

<sup>4</sup> I had conjectured this myself (in *Class. Phil.* XXV. [1930], p. 360), without having noticed the confirmation supplied by Quintus (cf. note 5 *infra*).

<sup>5</sup> Q. XII. 157 sqq.

<sup>6</sup> hic aliud maius miseris multoque tremendum obicitur magis atque improvida pectora turbat.

Laocoon . . . (V. II. 199 sqq.)

hic aliud at V. II. 199 is of course echoed by his aliud at V. XII. 244.

<sup>7</sup> The only other portent which can be found is the discovery of the wooden horse itself by the Trojans (J. W. Mackail, *The Aeneid of Vergil* [Oxford, 1930], *ad loc.*). But the comparison is then strained; and it has usually been supposed that there is a difficulty.

<sup>8</sup> τῷ δ' ἐπὶ κύντερον ἄλλο θεὰ μεγάθυμος Ἀθήνη δασύρηνος τέκτεσσαν ἐμήδετο Λαοκῶντος.

(Q. XII. 447 sq.)

Quintus had first been punished for his warning by a painful blindness<sup>1</sup>: but then, since he persisted, he was punished again by the death of his children. The mention of a still more frightful thing is appropriate enough to a second punishment. Vergil must have left out part of the story which he inherited, without perfectly adjusting the rest.

Both the Vergilian passages concerned with Laocoon interrupt a sequence of events otherwise exactly reproduced by Tryphiodorus,<sup>2</sup> who does not mention Laocoon. It is clearly probable therefore that Vergil, following a narrative used also by Tryphiodorus, superimposed on it a version of Laocoon's warning and death adopted partly from another source, used also by Quintus.

According to Tryphiodorus, the Trojans were warned, not by Laocoon before the horse was taken into Troy, but by Cassandra afterwards.<sup>3</sup> At this point in the narrative, Vergil, in two lines only, which like another reference to Cassandra have a provisional or unfinished aspect,<sup>4</sup> relates that 'then also' Cassandra prophesied.<sup>5</sup> Apparently Vergil reduced his treatment of Cassandra here to two temporary lines, and probably also made adjustments at his reference to Coroebus<sup>6</sup> later in the book, after he had decided to adopt the incident of Laocoon from some source used afterwards by Quintus, to strengthen it by emotional accessions drawn from Cassandra, and then to impose it on a narrative derived from a source of Tryphiodorus. Verbal similarities confirm the probability. To the source used by Tryphiodorus Vergil must owe the consequences of the alternative plans for dealing with the horse, which he and Tryphiodorus<sup>7</sup> alone record; though there are even then significant differences between the versions.

Vergil's Laocoon is the more likely to be derived from some authority of Quintus, partly because there are verbal similarities,<sup>8</sup> but partly also because the version of Quintus, itself a combination,<sup>9</sup> is nearer in form to Vergil's than any other which is known. For his own combination, Vergil has used one of the traditions followed by Quintus, and transferred motives from the action of Cassandra.<sup>10</sup> The

<sup>1</sup> Q. XII. 395 sqq. There was also an earthquake.

<sup>2</sup> Cf. V. II. 39 with T. 250 sqq. (where the Trojans deliberate what to do with the wooden horse): V. II. 57 sqq. with T. 256 sqq. (where Sinon is introduced): V. II. 194 with T. 303 (where Sinon's speech ends): V. II. 232 sqq. with T. 304 sqq. (where the horse is taken into Troy). The episode of Laocoon is contained in V. II. 40-56 and 199-231 (195-198 only relate that Sinon was believed by the Trojans).

<sup>3</sup> T. 358.

<sup>4</sup> V. II. 402 sqq. *illis nam Troiam forte diebus* (V. II. 408) and the unfinished line *audierit* (411) seem to betray partial adjustment. Cf. also *... lumina, nam teneras arcebant uincula palmas* (406), which looks like an adjustment to some version in which Cassandra lifted her hands, as at Q. XIV. 436 sqq., as well as her eyes to heaven. V. II. 403 sqq. seems in fact to contain a combination of elements found at Q. XII. 535 sqq. and XIV. 436 sqq. (both of Cassandra).

<sup>5</sup> V. II. 246 sq.

<sup>6</sup> V. II. 341 sqq. ~ Q. XIII. 174 sqq. For the death of Coroebus cf. V. II. 424 sqq.: Q. XIII. 168 sqq. Only in Quintus (XIII. 168 sqq.) and the *Ilias parva* (according to Proclus) is Coroebus killed by Diomedes. Possibly the *Ilias parva* itself is the source of Quintus here. Vergil, how-

ever, could not make Diomedes kill Coroebus, who is for Vergil a very sympathetic character, because Diomedes is friendly to Aeneas afterwards (V. XI. 243 sqq.). Nor could he allow the deed to be done by Neoptolemus (as in the general version), for that would have mitigated the tremendous entry of Neoptolemus, on the heels of Polites. He chose instead Peneleos (V. II. 425), a poetical name from one of his available traditions: for Peneleos appears with other Vergilian names at T. 180.

<sup>7</sup> V. II. 189 sqq.: T. 296 sqq.: cf. Cesareo, pp. 293 sqq.: Heinze, p. 81: cf. p. 68.

<sup>8</sup> Cf.: Q. XII. 390 sqq. ~ V. II. 35 sqq. and 43 sqq.: Q. XII. 395 sq. and 564 sq. ~ V. II. 54 sq.: Q. XII. 178 and 412 ~ V. II. 222: Q. XII. 407 sq. ~ V. II. 210 (where Vergil has apparently transferred the description from the eyes of Laocoon to the eyes of the snakes): Q. XII. 449 ~ V. II. 21: Q. XII. 457 ~ V. II. 209: Q. XII. 458 ~ V. II. 211: Q. XII. 461 sqq. ~ V. II. 212 sqq.: Q. XII. 476 sq. ~ V. II. 216 sq.

<sup>9</sup> Heinze, p. 68. He explains the reduplication of Laocoon's punishment as an adjustment by Quintus himself. Cf. Robert, pp. 192 sqq.: 200 sqq.: 209.

<sup>10</sup> Q. XII. 525 sqq.



attempt to damage the horse is attributed by Quintus to Cassandra but by Vergil to Laocoon, with verbal similarities.<sup>1</sup> Vergil has also added an element from the motive of Sinon. According to Quintus, the Trojans thought that Laocoon had been punished for their offence against Sinon, but, according to Vergil, for his own sin against the wooden horse: again the phrases are alike.<sup>2</sup> It is to be supposed that Vergil, having transferred the attempt to damage the horse from Cassandra to Laocoon, and having refrained from allowing his Trojans to act very cruelly towards Sinon, has transformed the guilt of harming Sinon into guilt of harming the sacred horse. The horse itself and its entry into Troy are described by both poets with similar expressions, but with changes of the usual kind.<sup>3</sup> There is still one transference to be recorded. Some words of Tryphiodorus suggest that the Vergilian descent of Laocoon from the citadel, which involves inconsistencies,<sup>4</sup> is modelled on the descent of Priam from the citadel in a former poem.<sup>5</sup>

There are signs that the character and actions of Cassandra, as they were in a source or sources of Quintus and Tryphiodorus, were further disintegrated by Vergil to create the characters of Helen and of Amata. This disintegration has left to Vergil's Cassandra two lines at the place where her warning might have been fully given, and two other short references.<sup>6</sup> In the Sixth *Aeneid* Helen leads the Trojan women in a bacchic dance—like Amata in the Seventh—holding up a burning torch, to summon the Achaeans.<sup>7</sup> In the second book Aeneas finds her hiding for fear of both Achaeans and Trojans alike at Vesta's threshold.<sup>8</sup> The two accounts have been thought incompatible—first probably by Vergil himself<sup>9</sup>—and Vergil's authority for the revelry of Helen has not been found. Now according to Tryphiodorus Athena ordered Helen, visiting the wooden horse in the night, to go back to her house and to await with a fire signal the ships of the Achaeans: and all night she displayed a torch.<sup>10</sup> This seems to represent some original of Vergil: but in Tryphiodorus Helen makes no revelry. On the other hand, Cassandra burst from her room and strayed like a bacchanal, shaking a laurel bough.<sup>11</sup> The remaining element is provided by Quintus, whose Cassandra takes a torch of blazing pine, not to make signals, but to set fire to the wooden horse.<sup>12</sup> This practically completes the Helen of the Sixth *Aeneid*: except that there she invited Menelaus to her house,<sup>13</sup> a detail possibly represented by the record in Homer and Tryphiodorus that she called to him

<sup>1</sup> Cf. Q. XII. 559 sqq.: 567 sq.: 571 sqq. with V. II. 31 sqq.: 36: 54 sq.: 229 sqq. The suggestions for destroying the horse given by Homer (θ 507 sqq.) and Arctinus (Procl. *epit. II. pers.*) are combined by Vergil. In this particular Quintus is nearer to Homer, and Tryphiodorus to Arctinus.

<sup>2</sup> Cf. Q. XII. 415 sqq. with V. II. 145: 228 sqq. Contrast *miserescimus ultro* at V. II. 145 with *ὄφελ' ἢ περ ὀκρελπαρres* at Q. XII. 422.

<sup>3</sup> Cf. Q. XII. 422 sqq. with V. II. 235 sqq.: 238 sqq.: 244 sqq. (cf. Serv. *ad loc.* and Q. XII. 523, later in the story): 248 sqq.

<sup>4</sup> E. Bethe in *Rhein. Mus.* XLVI. (1891), pp. 512 sqq.

<sup>5</sup> Cf. (in their contexts) T. 242 sq. with V. II. 40 sq.

<sup>6</sup> V. II. 246 sq.: 341 sqq.: 402 sqq.

<sup>7</sup> V. VI. 517 sqq. (of Helen: cited by Heinze [p. 79] and Cesareo [p. 275, note 1], but without, in my opinion, satisfactory conclusions): V. VII. 385 sqq.: 397 sq. (of Amata: cf. especially V. VII. 397 sq. with Q. XII. 568 sq.).

<sup>8</sup> V. II. 567 sqq.: cf. with 571 sqq. Q. XIII.

386 sq.: T. 630 sqq.

<sup>9</sup> Heyne, Vol. II., p. 256 sq., etc. Heinze (p. 45 and note 1) partially recognized this supreme instance of the Vergilian transmutation, for he realized that the traditional meeting of Menelaus with Helen is remembered: but he attributed it, according to the views of his time, to an unknown interpolator. The passage is intensely Vergilian and the account of Servius is quite satisfactory (cf. H. F. Fairclough in *Class. Phil.* I. [1906], pp. 221 sqq., etc.: and now E. Adelaide Hahn in *Class. Weekly*, XXV. [1931-1932], p. 60, note 11, cont. p. 61). The lines are restored by recent editors. If Vergil rejected them for insufficient reasons, there is a parallel in English literature: for Wordsworth removed some important lines from the *Ode on Intimations of Immortality*, persuaded by Coleridge, who was unable to understand them.

<sup>10</sup> T. 495 sq.: 512 sq.

<sup>11</sup> T. 365 sq.: 369 sq.

<sup>12</sup> Q. XII. 565 sqq.

<sup>13</sup> V. VI. 525.



while he was in the horse<sup>1</sup>; and that she took away the weapons of Deiphobus,<sup>2</sup> a particular in which Vergil also seems to be remembering the *Odyssey*.<sup>3</sup> The Helen of the Second *Aeneid* may have been developed from the same sources, but with less change. According to Quintus, Menelaus killed Deiphobus, finding him asleep by the bed of Helen, who was hiding in the hall for terror.<sup>4</sup> This incident brackets the Second *Aeneid* with the Sixth, where the account of Deiphobus is verbally like the same passage of Quintus.<sup>5</sup> However, there Vergil has reported Deiphobus, not, as Quintus almost certainly meant to say, heavy with wine,<sup>6</sup> but heavy with sleep, and overcome with cares. If the opinion which Aeneas gives in the same Vergilian context is right, Deiphobus, although Helen had taken away the arms, fell, when at last he was overpowered, on a great heap of the dead, apparently slain by himself.<sup>7</sup> This is the Homeric account<sup>8</sup>: but it is inconsistent here with what follows.

The similarities of phrase and incident, and the difficulties in Vergil's narrative, can be explained in general as follows. Vergil has in his sixth book blended together the Cassandra of a source used by Quintus with the Cassandra of a different source used by Tryphiodorus; and has then, influenced perhaps also by the fire signal of Sinon in the cyclic epic,<sup>9</sup> transferred actions from his composite Cassandra, who had already lost other actions to Laocoon, to the Helen whom he had adopted from both these sources; a Helen who appears, less changed and without the attributes of Cassandra, in the Second *Aeneid*.

The meeting of Aeneas with Helen, recorded by Vergil, can also be resolved into elements belonging to these traditions. Aeneas, seeing Helen at Vesta's threshold,<sup>10</sup> thinks of killing her. But at that moment his divine mother appears and restrains him with her hand and her words<sup>11</sup>: and shows him in a vision the gods themselves, helping in the destruction of Troy.<sup>12</sup> Quintus gives in full the meeting of Menelaus and Helen<sup>13</sup> assumed in the *Odyssey*,<sup>14</sup> but adds also the intervention of Aphrodite.<sup>15</sup> Menelaus finds Helen in terror. At first he wants to kill her; but Aphrodite restrains him, and reminds him of Helen's beauty.

There are transferences of detail: and the recognition of them often illustrates the utility of Vergil's method. When Aeneas is about to kill Helen, the thought occurs to him that there is no glory in killing a woman.<sup>16</sup> Vergil appears to owe this thought to some original of a passage of Quintus, in which Ilioneus, in an appeal to Neoptolemus, says that there is no glory in killing an old man.<sup>17</sup> In the *Aeneid* Ilioneus survives.<sup>18</sup> There seems to be a particularly Vergilian transmutation in the words of Venus to Aeneas. She tells him that he must blame neither Helen nor Paris, for the gods have overthrown the city.<sup>19</sup> Quintus has verses in form and

<sup>1</sup> T. 469 sqq.: cf. δ 274 sqq.: but the incident is not entirely furnished to Tryphiodorus by Homer.

<sup>2</sup> V. VI. 523 sq.

<sup>3</sup> τ 4 sqq.: 31 sqq. Vergil may have been influenced by 33 sq., where Athena shows a supernatural light, and in general by λ 424 sqq.

<sup>4</sup> Q. XIII. 354 sqq.

<sup>5</sup> V. VI. 520 sqq.

<sup>6</sup> With καρηβαρύνοντα at Q. XIII. 355 cf. βεβαρημένος οὖν at Q. XIII. 164, and in general the condition of the Trojans in Quintus.

<sup>7</sup> V. VI. 502 sqq.

<sup>8</sup> θ 517 sqq.

<sup>9</sup> Procl. *epit. Il. pers.*: T. 510 sq.

<sup>10</sup> V. II. 567 sqq.: cf. T. 630 sqq.

<sup>11</sup> V. II. 588 sqq.

<sup>12</sup> V. II. 604 sqq.

<sup>13</sup> Q. XIII. 385 sqq.

<sup>14</sup> θ 514.

<sup>15</sup> Stesichorus was probably the first to mention the personal intervention of Aphrodite (Robert, pp. 69 sqq.). Johannes Tzetzes (τὰ μετ' Ὀμηρον, 750) possibly indicates that his work survived late, though hardly to the date of Tzetzes. Stesichorus may have influenced Vergil: who seems, however, also to remember the meeting of Aphrodite with Anchises in the form adopted by Sophocles (Dion. Hal. *Ant. Rom.* I. 48) and probably Ennius (E. M. Steuart, *The Annals of Ennius* [Cambridge, 1925], p. 8 [Ann. I. frs. 9, 10, 11]: cf. pp. 103 sqq.).

<sup>16</sup> V. II. 538 sqq.

<sup>17</sup> Q. XIII. 192 sqq.

<sup>18</sup> V. I. 521, etc.

<sup>19</sup> V. II. 601 sqq. The strange syntax of *non . . . culpatus* may be a sign that Vergil was attempting to follow the verse form of an original closely.

language very much like these verses of Vergil, in which Aphrodite warns Menelaus that Paris and not, as he thinks, Helen is to blame<sup>1</sup>: of course, not here for the ruin of Troy, but for the losses and hardships of Menelaus and the other Achaeans. This instance seems to be decisive for the theory of transference. There are other reminiscences in the vision of Aeneas. He sees masonry violently dislodged, and clouds of mixed dust and smoke,<sup>2</sup> where Neptune is at work. Quintus retains the same association,<sup>3</sup> but in plain narrative and without the agency of the god.

In this context<sup>4</sup> the theory of transference solves a difficulty. At the destruction of Troy Tryphiodorus supplies the mention of divine agency<sup>5</sup> lacking to Quintus. But the gods appeared also, according to Tryphiodorus, to help or welcome the wooden horse<sup>6</sup> when the Trojans drew it through their gate. It has been thought that Tryphiodorus may have adopted the scene from Vergil and duplicated it.<sup>7</sup> It is more likely that Vergil found both appearances of the gods in a source of Tryphiodorus, and compressed them into one.<sup>8</sup> A reason for believing this is that a description of the action of Athena in Tryphiodorus, when she helps forward the wooden horse towards the gate, has a close parallel in the Tenth *Aeneid*.<sup>9</sup> Another reason is this. It has been noticed<sup>10</sup> that in the *Aeneid*, though, hours before, Androgeos has blamed the disguised Trojans for being late in arriving from the ships,<sup>11</sup> nevertheless Juno is still summoning Achaeans in the vision of Aeneas.<sup>12</sup> Her action seems to belong to a previous activity at the entry of the wooden horse. That would explain the difficulty.

The method of resolution by elements has already<sup>13</sup> been partly applied to the Vergilian death of Priam.<sup>14</sup> The late epic<sup>15</sup> mainly follows the usual<sup>16</sup> version, the version of the *Iliu persis*, with which Vergil has combined the variant of the *Ilias parua*. However, Vergil, though he definitely says that Priam was stabbed,<sup>17</sup> agreeing apparently with Tryphiodorus,<sup>18</sup> soon afterwards writes as if Priam had been beheaded.<sup>19</sup> This second version, usually traced to Pacuvius,<sup>20</sup> was chosen by Quintus: according to whom Priam's head rolled far along the ground, away from his other members, in the blood of other men.<sup>21</sup> This allusion reappears in a typical transmutation: in the *Aeneid* it is the blood of Priam's own son, slain before his eyes, and Priam slides in it before, not after, his death.<sup>22</sup>

There is a consequential transmutation of the death of Polites. According to Quintus, he was killed attacking, not fleeing, with a cast of a spear, apparently out

<sup>1</sup> Q. XIII. 412 sqq.: cf. P 164 sqq.

<sup>2</sup> V. II. 608 sqq.

<sup>3</sup> Q. XIII. 430 sqq.: cf. V. II. 310 sqq.: 483 sq.: 501 sqq.: all three Vergilian passages seem to share elements with Q. XIII. 430 sqq.

<sup>4</sup> At V. II. 604 sqq. (where Venus withdraws the cloud from the eyes of Aeneas) originals of Q. XIII. 415 sqq. and of T. 310 sq., besides Ennius, *Ann.* I., fr. 9 (Steuart), and E 127, seem to have contributed influence.

<sup>5</sup> T. 566 sqq.: cf. in this context T. 574 sq.: 681 with V. II. 606 sqq.: 624 sq.: III. 3.

<sup>6</sup> T. 336 sqq. According to Vergil the horse halted after the demolition of masonry at the gate of Troy (V. II. 234: 242 sq.), not before, as in Tryphiodorus, whose account appears nearer to the facts. Cf. my forthcoming article in *Class. Journ.* <sup>7</sup> By Castiglioni (p. 504).

<sup>8</sup> V. II. 608 sqq. There may be influence on Vergil and Tryphiodorus from T 31 sqq.: but they are nearer to each other than to Homer: cf. Heinze, pp. 51 sqq.

<sup>9</sup> Cf. T. 33 sqq. with V. X. 246 sqq.

<sup>10</sup> By Heinze (p. 52).

<sup>11</sup> V. II. 370 sqq.

<sup>12</sup> V. II. 613 sq.

<sup>13</sup> Heinze, pp. 42 sqq.

<sup>14</sup> V. II. 506 sqq.

<sup>15</sup> Q. XIII. 220 sqq.: T. 634 sqq.: cf. V. II. 550 sqq.

<sup>16</sup> Pausanias, X. 27. 1 sq. (1).

<sup>17</sup> V. II. 553.

<sup>18</sup> T. 635.

<sup>19</sup> V. II. 557 sq.

<sup>20</sup> Serv. ad Verg. *Aen.* II. 506: 507. Vergil may have intended a comparison with the fate of Pompeius Magnus (Guillemin, pp. 60 sq.: cf. Lucan, *Phars.* VIII. 708 sqq.): or an allegory, as H. Schickinger suggested (in *Wiener Studien*, XXVIII. [1906], pp. 165-167 cf. J. D. Meerwaldt in *Mnemosyne*, LX. [1931], pp. 184 sqq., where the view is defended).

<sup>21</sup> Q. XIII. 241 sqq.

<sup>22</sup> V. II. 550 sqq.

of sight of Priam.<sup>1</sup> Vergil relates that he was pursued, wounded with the point of Neoptolemus' spear, to die at Priam's feet.<sup>2</sup> There are other communities of influence, but even more than usually the *ethos* is greatly different.<sup>3</sup> However, Vergil and Quintus both add, immediately after Priam's death, lines in contemplation of his tragic fate<sup>4</sup> which suggest a common source: and Vergil and Tryphiodorus share a repudiation by Neoptolemus of the mercy which Achilles showed to Priam, and a reference to the retribution which awaited Neoptolemus at Delphi.<sup>5</sup>

There are suggestive similarities in the stories of the night-fighting in Troy. Vergil records a moment when the fortune of the fight seemed to change: the Trojans regained courage and the victorious Danai fell.<sup>6</sup> This appears in Quintus. There is less emphasis: and Quintus says that the Danai were not unwounded, but that some were struck by wine cups and some were injured in other ways.<sup>7</sup> Here again Vergil seems to have inherited something like the version of Quintus, and to have carefully removed from his own a reference to the drunkenness of the Trojans. When in the *Aeneid* the disguised Trojans are sustaining losses from the weapons of their own compatriots, the holiness of Panthus does not preserve him from death; and in the same context Rhipheus, the most just of all the Trojans, is not saved by all his goodness.<sup>8</sup> Now Quintus matches these Vergilian motives at the death of Eurydamas<sup>9</sup>: and his poetry seems to represent an original which Vergil has expanded, duplicated, and infinitely enriched. The episode of Androgeos, who was killed by foes disguised as friends,<sup>10</sup> is connected with a difficulty<sup>11</sup> which the theory of transference explains. Tryphiodorus has an episode like it in which such a mistake is made,<sup>12</sup> but it is far less concrete and individualized. The original of Tryphiodorus may have been used by Vergil at a different place. The defence from the roof of a building is related by Vergil, who includes it in his story of the change of arms, and by Tryphiodorus,<sup>13</sup> with similar expressions. Tryphiodorus here describes a mysterious manoeuvre of the attackers with words that have invited emendation.<sup>14</sup> Vergil seems to have identified the same movement with the Roman *testudo*, showing his characteristic ingenuity. Lastly, Tryphiodorus and Vergil both interpose a comment on the impossibility of describing all the horrors of the night-fighting<sup>15</sup>—horrors related by Quintus in a detailed description<sup>16</sup> such as Vergil seems to have known, but discarded with three words.<sup>17</sup> As usual it is hard to doubt that

<sup>1</sup> Q. XIII. 213 sqq.

<sup>2</sup> V. II. 526 sqq. Vergil seems to have found in an original at this place a reference to the sons of Priam and to the death of Polites by a spear, and to have combined an incident, preserved at Q. VIII. 409 sqq., in which Polites escapes an arrow. His use of these ideas is typical.

<sup>3</sup> V. II. 533 sqq.: Q. XIII. 225 sqq.: T. 636 sqq.

<sup>4</sup> V. II. 554 sqq.: Q. XIII. 246 sqq.: cf. also 543 sqq. and X 61 sq.: Q 255 sqq.

<sup>5</sup> V. II. 536 sqq.: T. 634 sqq. Cf. with T. 635 V. II. 509 (of Priam) and 596 (of Anchises): with T. 634 sqq. T. 231 sqq. and V. II. 540 sqq. (for the reference to Achilles and Hector): and with Q. XIII. 232 sq. V. II. 555 sq.

<sup>6</sup> V. II. 366 sqq.

<sup>7</sup> Q. XIII. 145 sqq.

<sup>8</sup> V. II. 426 sqq.

<sup>9</sup> Q. XIII. 178 sqq.

<sup>10</sup> V. II. 370 sqq.: 376 sqq.: cf. 410 sqq.

<sup>11</sup> Androgeos is otherwise unknown in the Trojan cycle (Heyne, *ad loc.*). When he meets Aeneas, his words indicate that the Achaeans are

already burning and pillaging Troy (V. II. 373 sqq.), though there has hardly been time for their work to have proceeded so far (Heinze, p. 27: cf. 73).

<sup>12</sup> T. 577 sqq.

<sup>13</sup> Cf. T. 619 sqq. with V. II. 409 sqq.: 438 sqq. Cf., with V. II. 440, Q. XIII. 85.  $\theta$  519 is apparently remembered at V. II. 438.  $\theta$  514 sq. proves, of course, that Homer gives extracts or short allusions from a longer poetry of the sack of Troy already existing.

<sup>14</sup> Castiglioni, p. 501.

<sup>15</sup> Cf. T. 664 sq. with V. II. 361 sq.: where an Ennian quality sustains the probability that Vergil owes much in the Second *Aeneid* to the suggestion of Ennius, with whom he also seems to share sometimes the same Greek originals.

<sup>16</sup> Q. XIII. 81 sqq.

<sup>17</sup> V. II. 368 sq. I add the suggestion that a common original was followed at V. II. 313, where trumpets are mentioned, and at T. 326 sq., where a supernatural trumpet prophesies war as the horse is drawn into Troy. For V. II. 313 Servius compares an ancient custom of taking

the Greek passage represents an original basis of thought, and that it is the Latin which has the artistic finality which genius has imposed on such material.

Quintus<sup>1</sup> has a remarkable description, epitomized by Tryphiodorus,<sup>2</sup> in which he records the departure of Aeneas from Troy. The comparison<sup>3</sup> is of some interest: and the transferences are significant. According to Quintus, Aeneas, leaving Troy when he saw how final was the destruction, noticed women and children being dragged from the buildings.<sup>4</sup> The women and children reappear in Vergil, but as captives waiting in long ranks<sup>5</sup> whom Aeneas sees when he returns to Troy in search of Creusa. Another transference is this. Quintus relates that Cypris led Aeneas out of Troy; the fire divided, and the weapons of the Achaeans fell harmless.<sup>6</sup> This is close to Vergil's account, not of the departure of Aeneas from Troy, but of his walk from Priam's palace to the house of Anchises.<sup>7</sup> In the Second *Aeneid* Vergil does not mention that Venus led Aeneas out of Troy, but only that he went to the house of Anchises under her guidance. But elsewhere Vergil seems to assume that Venus did in fact guide Aeneas from Troy.<sup>8</sup> It is clear that Vergil has transferred this guidance of Venus from a later to an earlier moment of the narrative. It was said that the star of Venus directed Aeneas on his journey.<sup>9</sup> This star seems to have become for Vergil the shooting star which persuaded Anchises to leave his home.<sup>10</sup> There is thus probably nothing left at the end of the Second *Aeneid* in its present draft to show that Aeneas was ever guided forth on his wandering by his divine mother, as expectation and Vergil's words elsewhere require.

The results of the comparisons have been unusual, perhaps because I have tried to include in them the whole of each epic, and to compare not two poems only together, but all three at once. The parallels are too many for coincidence, and some of them carry conviction in their own right. But they cannot be explained on the theory—of which they have been the only support—that the late epic is directly dependent on Vergil; for the differences between the three texts are great, and of a significant kind. If the dates at which the poems were composed had been entirely unknown, it would long ago have been agreed that the *Aeneid* partly represents a conflation of the other two.<sup>11</sup> The late epic is unquestionably nearer to direct descent

cities to the sound of trumpets (recognized apparently at Σ 219): and, during an investigation of the magically regarded incidents of the fall of Troy, I had compared the trumpets used at Jericho for T. 326 sq. (in *Class. Phil.* XXV. [1930], p. 363, note 2). Cf. the conjecture of R. H. Klausen (*Aeneas und die Penaten* [Hamburg, 1840], Vol. II., p. 693) that in the early saga the Heracleidae took Argos in this way: a conjecture which O. Gruppe (*Griechische Mythologie und Religionsgeschichte* [München, 1906], Vol. II., p. 1199 and note 4) thought bold but probably right. Tryphiodorus should have connected the trumpet call with Athena (cf. Gruppe, *ibid.*, for the storm-symbolism of her aegis and the explanation that the trumpets were meant to exert wind pressure on the walls), and with the effective breach of the magic wall to admit the horse (cf. *reft.* at p. 181, note 6, *supr.*: T. 302: 330 sqq.: 390): but he attributes the call to Zeus, and considers it only prophetic. Vergil has suppressed these significances, as he has rationalized, apparently, at V. II. 298 a personification of the magic walls, preserved at Q. XII. 510 (where, if I am right,

the MSS. reading should be restored), probably from the same original.

<sup>1</sup> Q. XIII. 300 sqq. Heinze (p. 72) shows that the passage cannot be derived from the Second *Aeneid*: but in my opinion handbooks or general knowledge will hardly account for the conditions, as Heinze thinks (pp. 72 sq.: 81).

<sup>2</sup> T. 651 sqq.

<sup>3</sup> Cf. e.g. Q. XIII. 320 sqq. with V. II. 723 sq. (of Ascanius).

<sup>4</sup> Q. XIII. 305 sq.

<sup>5</sup> V. II. 766 sq.

<sup>6</sup> Q. XIII. 325 sqq.

<sup>7</sup> V. II. 632 sq.: cf. 459.

<sup>8</sup> V. I. 381 sq. For the difficulty cf. Catharine Saunders, *Vergil's Primitive Italy* (Oxford and New York, 1931), pp. 199 sqq. (=C.Q. XIX. [1925], pp. 85 sqq.).

<sup>9</sup> Serv. *ad Verg. Aen.* I. 381, citing Varro.

<sup>10</sup> V. II. 692 sqq. That Lucifer at V. II. 801 should be connected with Venus was supposed by Heyne (Vol. II., p. 336) after Servius.

<sup>11</sup> Cf. e.g. the incident of Sinon: V. II. 57 sqq. looks like a conflation of Q. XII. 360 sqq. and I. 258 sqq.



from the earlier Greek tradition: and the differences which Vergil shows are explicable by the nature of his art and the requirements of his plan. The probability, already overwhelming, that Vergil is not the source of the late poets is confirmed: but the use by him and by them of a common poetic tradition appears to be established. Differences between the late poets themselves, especially when one of them shows agreement with Vergil, and most of all the differences which correspond with inconsistencies in the *Aeneid*, seem to require the further inference that there were two or more distinct sources, both or all accessible to Vergil, and one or more, but not always the same source or sources, accessible respectively to Quintus and Tryphiodorus. I do not here attempt to identify these lost poems. And I only indicate lightly the power and brilliance of Vergil's art, which the transferences and transmutations help to disclose.

W. F. J. KNIGHT.

BLOXHAM SCHOOL.

## THE DATE OF CTESIBIUS.

THE question of the date of Ctesibius has been much obscured of late years by those German scholars<sup>1</sup> who assert that Ctesibius the pneumatic and hydraulic engineer mentioned by Vitruvius IX. 8. 2 is distinct and separate from Κτησίβιος ὁ μηχανικός who is mentioned by Athenaeus Mechanicus (Βελοποιικά, p. 8, Thevenot), Philo of Byzantium (pp. 56, 67, 72, 77, Thevenot), and Hedylus *ap.* Athenaeum Naucraticum *Deipn.* XI., p. 497, d-e = Anthologia Graeca ed. Cougny, Paris (Didot), 1890, Vol. III., p. 298, n. 67.

Ἡδύλος δ' ἐν ἐπιγράμμασι περὶ τοῦ κατασκευασθέντος ὑπὸ Κτησιβίου τοῦ μηχανοποιοῦ ῥυτοῦ μνημονεύων φησί·

Ζωρόποταί, καὶ τοῦτο φιλοξεφύρου κατὰ νηδὺν  
τὸ ῥυτὸν εὐδίας δεῦτ' ἴδεν' Ἀρσινόης  
ὀρχηστὴν Βησῶν Αἰγύπτιον· ὃς λιγὺν ἦχον  
σαλπίζει κρουνοῦ πρὸς ῥυσὶν οἰγομένου,  
οὐ πολέμον σύνθημα, διὰ χρυσέου δὲ γέγωνεν  
κώδωνος κώμον σύνθεμα καὶ θαλίσ,·  
Νεῖλος ὁκοῖον ἀναξ μύσταις φίλον ἱεραγωγοῖς  
εὔρε μέλος θείων πάτριον ἐξ ὑδάτων.  
ἀλλ' εἰ Κτησιβίου σοφὸν εὔρεμα τίετε τοῦτο,  
δεῦτε, νέοι, νηῶ τῷδε παρ' Ἀρσινόης.

This epigram is quoted in the *Deipnosophistae* apropos of a statement (p. 476 b fin.) that Ptolemy II. Philadelphus (285-247) made ῥυτά or horns, drinking vessels not cornucopiae, attributes, *φορήματα*, of statues of Arsinoe: and Ctesibius is clearly thought of as the inventor of these ῥυτά for the adornment of Arsinoe. The particular one in the epigram is ornamented with a figure of the Egyptian god Bes dancing.

Now Arsinoe II., wife of Ptolemy II. Philadelphus, on the fine silver decadrachms of Alexandria issued after her deification in 270 B.C., has a double cornucopiae with the inscription ΑΡΣΙΝΟΗΣ ΦΙΛΑΔΕΛΦΟΥ, and the same occurs on her golden octodrachms; cf. B. V. Head, *Historia Numorum*, Ed. II., p. 850. The normal type of Nilus on coins of Alexandria is a recumbent male figure also with a cornucopiae: see the British Museum Catalogue of Greek Coins: Alexandria, Plate XXI. It seems, then, as if Ctesibius' ῥυτά are an improvement on the cornucopiae of Arsinoe.

The couplet

Νεῖλος ὁκοῖον ἀναξ μύσταις φίλον ἱεραγωγοῖς  
εὔρε μέλος θείων πάτριον ἐξ ὑδάτων

is obscure.

Haec de sistro inuento interpretatur Casaubonus: uix apte. Hoc apparet, fluminis cum dulci strepitu ripas praeterlabentis murmur describi: in eo autem murmure quid sit quod ad mystas pertineat equidem ignoro, says Jacobs, *Anthologia Graeca*, Vol. VII., p. 338. Jacobs is right; the sistra are not indicated. The reference is probably to the sound made by the rising water of the Nile entering a Nilometer.

<sup>1</sup> Susemihl, *Gesch. d. Lit. d. Alex.-Zeit.* I. 734<sup>182</sup>. W. Schmidt, *Hero Alexandrinus Einleitung*, p. X., n. 1. Wilamowitz and Kaibel ad

*Ath. Deipn.* IV. 174d. Diels, *Ant. Technik.* (Berl.-Lpz., 1920), 198. 3 and 203. 3.

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The *μύσται* *ιεραγωγοί* are then the celebrants of the *Νεῖλῳα*. This was a festival held at the summer solstice when the river began to rise. Heliiodorus *Aeth.* IX. 9. καὶ γάρ πως συνέπεσε καὶ τὰ *Νεῖλῳα* τότε τὴν μεγίστην παρ' Αἰγυπτίοις ἑορτὴν ἐνεστηκέναι, κατὰ τροπὰς μὲν τὰς θερινὰς μάλιστα, καὶ ὅτ' ἀρχὴν τῆς αὐξήσεως ὁ ποταμὸς ἐμφαίνει τελουμένην, ὑπὲρ πάσας δὲ τὰς ἄλλας πρὸς Αἰγυπτίων σπουδαζομένην. A Nilometer into which the water came gurgling through underground passages is described in Heliiodorus IX. 22: τὸ ποτάμιον ὕδωρ ὑπὸ γῆς διηθούμενον καὶ ταῖς γραμμαῖς ἐμπύπτον τὰς τ' αὐξήσεις τοῦ Νείλου καὶ ὑπονοστήσεις τοῖς ἐγχωρίοις διασημαίνει.

To return to Ctesibius; the epithet *μηχανοποιός* sorts well with Athenaeus *Mechanicus*, p. 8, ὁ *μηχανικός*, and with Vitruvius IX. 8. 4, *aquarum expressiones automato-poetasque machinas multaque deliciarum genera explicuit*. But, say the Germans, the Ctesibius mentioned in Vitruvius must be carefully distinguished from the one mentioned in Athenaeus *Mechanicus*, in Hedylus' epigram, and in Philo Byzantius. This despite the fact that Vitruvius calls his Ctesibius in IX. 8. 2 the founder of pneumatic science: *qui et uim spiritus naturalis pneumaticasque res inuenit*, and Philo and Hedylus mention pneumatic devices. In particular Philo (p. 77, Thevenot) describes in detail Ctesibius' original experiments on the mechanical properties of air and (p. 78) a pneumatic catapult made by Ctesibius as a result of his discoveries.

Ctesibius, son of a barber, of Alexandria, named by Vitruvius IX. 8. 2, say these German critics, is to be identified with the man mentioned by Aristocles *ap. Athenaeum Naucratis Deipn.* IV., p. 174c. In that place the discussion is of hydraulic organs, and Aristocles is quoted on the subject. ἡ ὑδραυλὶς . . . 'Ἀλεξανδρέως ἐστὶν ἡμεδαποῦ εὐρημα κούρεως τὴν τέχνην'. Κτησίβιος δ' αὐτῷ τοῦνομα . . . ; then comes the quotation from Aristocles, *περὶ χορῶν*. 'καὶ φασι,' says Aristocles, 'τοῦτο εὐρησθαι ὑπὸ Κτησιβίου κούρεως ἐνταῦθα οἰκούντος ἐν τῇ Ἀσπενδίᾳ ἐπὶ τοῦ δευτέρου Εὐεργέτου, διαπρέψαι τέ φασι μεγάλως.' . . . Athenaeus resumes, *Τρύφων δ' ἐν τρίτῳ περὶ ὀνομασιῶν, ἐστὶ δὲ τὸ σύγγραμμα περὶ αὐλῶν καὶ ὀργάνων, συγγράφαι φησὶ* (fr. III V.) *περὶ τῆς ὑδραυλὸς Κτησίβιον τὸν μηχανικόν. ἐγὼ δὲ οὐκ οἶδα εἰ περὶ τὸ ὄνομα σφάλλεται*.

Now the date of Ptolemy Euergetes II. Physcon (died 117 B.C.) is sufficient to show that the Ctesibius mentioned here cannot be the same as the one in Athenaeus *Mechanicus*, Philo and Hedylus. Yet if the obstacle raised by this passage of Aristocles could be removed, the unity of Ctesibius would be certain.

And the obstacle is easily removed. Instead of *ἐπὶ τοῦ δευτέρου Εὐεργέτου* read *ἐπὶ τοῦ βασιλέως Εὐεργέτου*. This is not such a violent change as might at first sight appear. Aristocles was a specialist writing on a technical subject. It is likely therefore that his papyri would contain many compendia and suspensions. We may compare the London Papyrus of Aristotle's *Ἀθηναίων πολιτεία*, the Fragmentum Bobianum Mathematicum, and the Berlin Didymus. It may well be then that at some point in the tradition of Aristocles a scribe was confronted by a suspension representing *βασιλέως* which he interpreted as *δευτέρου*. For instance ΕΠΙΤΟΥ-ΒΕΥΕΡΓΕΤΟΥ. There is ample evidence that *βασιλεύς* and its derivatives were represented by suspensions of this kind. See the Catalogue of Papyrus in the British Museum, Vol. V., p. 323; and especially Vol. III., p. 70 sqq., Papyrus No. 604a. In this papyrus, which is of 46-47 A.D., the word *βασιλικῆς* appears well over one hundred times written as either *βασι<sup>a</sup>* or *B<sup>a</sup>*. More frequently, one hundred times in fact, it is *B<sup>a</sup>*. That such suspensions were recognized and accepted for the word *βασιλεύς* is proved by the fact that they persist in mediaeval Greek manuscripts. Compare Gardthausen, *Griechische Palaeographie*, Ed. II. (1911), Vol. II. (1913), p. 344, col. 3. *β<sup>a</sup>* = *βασιλεῖ*, *βασιλεῖς*. *β<sup>i</sup>* = *βασιλικός*, *β<sup>a</sup>* = *βασιλεία*, etc. *βασιλεύς* is also often found on coins reduced to *B*. See Rasche, *Lexicon Universae Rei Nummariae*, Vol. I., col. 1406; Suppl. I., col. 1282, where are many examples.

Now Ptolemy III., ὁ βασιλεὺς Εὐεργέτης, followed Ptolemy II. Philadelphus to the throne in 247 B.C., and it is reasonable to suppose that our now unified Ctesibius survived from the reign of Ptolemy II. Philadelphus into that of Ptolemy III. Euergetes. The doubt expressed by Athenaeus whether Trypho was correct is now a doubt whether the one and only Ctesibius the Engineer wrote on hydraulic organs or no. It is not a doubt whether it was the fictitious Ctesibius the Barber, invented by the Germans, or Ctesibius the Engineer.<sup>1</sup>

On this point, whether Ctesibius wrote on the ὑδραυλὶς, Trypho appears to be right, Vitruvius IX. 8. 4. Ctesibius . . . hydraulicas machinas primus instituit.<sup>2</sup> These words of Vitruvius, along with IX. 8. 2, and Athenaeus *Naucratis Deipn.* IV., p. 174 c. sqq., may here be opportunely used to correct the error propagated by W. Schmidt in his preface to the Teubner *Hero of Alexandria*. For on p. xi in note i. to p. x he remarks in the face of these two authorities that Ctesibius the Barber only improved the Hydraulis.

E. J. A. KENNY.

TRINITY COLLEGE, CAMBRIDGE.

<sup>1</sup> Cf. Martin, *Mém. Prés.* IV. 23. Paris, 1854.  
V. Rose, *Anecdota Graeca et Graecolatina*, II.  
283. Tannery, *Revue des Études Grecs*, IX. 23.

Christ-Schmid, *L.G.* II. i. 283.  
<sup>2</sup> Cf. VII. *Praef.* 14.

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KENNY.

## NEW LIGHT ON FESTUS.

In Italy, at the end of the tenth century, a pedant named Regulus (?) who had a copy of the *De Verborum Significatu* (or had made extracts from one), wishing to read Plautus (so often quoted by Festus), took the opportunity of an illness to appeal to certain prelates whose church-library contained a MS. of the comedian. Through their stupidity he received not Plautus, but Plato, i.e. Chalcidius' translation of the *Timaeus*. Disappointed, but not deterred, he wrote the following letter (in a sort of rhyming prose, affected by the *litterati* of that time) on the fly-leaf and returned the MS. (now Bamberg. Class. 18), hoping that by much repetition he might hammer into their dull heads the difference between PL-AU-TUS and PL-A-TO and yet save them from chagrin and resentment (the material for the letter was supplied by Festus, although the opening illustration comes from Chalcidius):

Differentia est qua differunt singula: in Timeo Platonis legitur ANIMA-M post ANIMA-L non perire: in Plauto legitur CANCRUM imitatis, item aeneis COCULIS excocta est mihi omnis misericordia.

Dum Plauti mea insipientia quesivit Asinariam,  
vestra sapientia Socratem, Timeum, Hermocratem, Critiam,  
non sine infertore Platone optimam  
mihi plus imperfecto quam egroto subministravit cenam.

Dum expectabam Plautum in Epidico  
salubres infirmitatis mee consolatores vos repperi in lecto.

And so on. The letter ends:

Habitet Plautus in Sinaristosis sine extortis talis,  
habitent semper mei domini in tutis et amoenis locis.

Let us hope that this crazy parade of learning conciliated the proud prelates, and that they sent Plautus at last to poor Uriah Heep Regulus. And let us be thankful that he wrote his letter in the MS. itself and not on a separate parchment; for from this accident we get a glimpse at a full text of the *De Verborum Significatu*, and learn such things as that Festus gave the line (Plaut. frag. 181) s.v. *Cocula* (34, 24), and that he cited the *Cistellaria* under its Greek title Συναριστώραι 'The Ladies at Breakfast' (from the opening breakfast-scene, the famous scene depicted, as Marx has shown in *Rhein. Mus.* LXXIX 197, in a mosaic of Pompeii).

It is apparently Lehmann's discovery. His pupil, Dr. B. Bischoff, has written a full account in *Philologus* (LXXXVII, pp. 114-117), to which Professor Ed. Fraenkel adds (pp. 117-120) some good remarks. Fraenkel suggests that the Naples Festus, our only surviving MS., had (at 390, 8 and 480, 23)<sup>1</sup> the symbol *Syna* or *Synar* for *Synaristosae*, but does not conceal the objection to this, viz. that titles of books are written in full in the Naples Festus.

<sup>1</sup> I always cite Festus from the small Teubner edition, in which the paging of Müller's large edition is also given. The *Thesaurus linguae Latinae* now cites from my large annotated Festus

in Vol. IV of my *Glossaria Latina* (Paris, 'Les Belles Lettres,' 1930). The paging of Müller is not mentioned there because this book is meant to supersede Müller.

There is method in the madness of this letter. It is Plautus, Plautus, Plautus who is quoted, and only Plautus. One sentence offers difficulty:

Post Delphicum Pacuvium  
vobis praesentibus non timeo adversitatis dolium.

It will offer none to our gallant conjectural emendators.<sup>1</sup> With their dog-matic divination they, *χαίροντες ὥσπερ σκυλάκια τῷ ἔλκειν τε καὶ σπαράττειν*, will turn it in a trice into *sicut Delphium Plautinum vobis praesentibus non timeo adversitantis dolium*.

ST. ANDREWS.

W. M. LINDSAY.

<sup>1</sup> The leader of this band recently (*C.R.* XLIV 115), after wrongly accusing Heraeus and me of not knowing that *draucus* meant 'a strong man,' dogmatically divined *dracti* (a word he had found in the new Liddell and Scott) in Martial XI viii, 1:

Lassa quod hesterni spirant opobalsama drauci.

Heraeus and I remembered—but he did not remember—*Mart. XIV lix, 1*:

Balsama me capiunt, haec sunt unguenta virorum.

Moreover, Heraeus and I knew—but he did not know—the Cyrillus gloss:

ΔΡΑΤΚΙΟΝ: monile.

## STELLA = SIDVS.

PROFESSOR HOUSMAN states (*C.Q.* IX, p. 33) that *stella* never is used to mean *sidus*, and for authors of the best age I believe he is right; at least I know of no examples except those which he convincingly explains away in the article quoted. There seem, however, to be instances of this usage perhaps as early as the age of the Antonines.<sup>1</sup> Hyginus, *fab. cxcv*, says of Orion, *ab Ioue in stellarum numerum est relatus, quam stellam Orionem uocant*. Again, *fab. ccxxiv*, *Crotos . . . in stellam Sagittarium*. Hyginus, it is true, was a fool, and his book is crammed with mistakes, including astronomical blunders; thus, just after the last-quoted passage, and also in *fab. cxxx*, he names the star Arcturus when he means the constellation Arctophylax, or Bootes. But I cannot think him quite so grossly ignorant, since he was interested in star-myths, as not to know that Orion and Sagittarius are constellations. It may be, however, that the use of *stella* in these passages is due, not to Hyginus himself, but to the epitomators whose extracts from the original work are all that has come down to us. Of these gentry I can believe almost anything that implies false Latin or general ignorance.

ST. ANDREW'S UNIVERSITY.

H. J. ROSE.

<sup>1</sup> Hyginus is known to have written before A.D. 207, and I suppose the composition of his work to fall within a century or so previous to

that date. See Rose, *Modern Methods in Classical Mythology*, p. 37.

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## ARISTOXENUS AND THE INTERVALS OF GREEK MUSIC.

ANCIENT Greek music was purely or predominantly melodic; and in such music subtleties of intonation count for much. If our sources of information about the intervals used in Greek music are not always easy to interpret, they are at any rate fairly voluminous. On the one hand we have Aristoxenus, by whom musical intervals were regarded spatially and combined and subdivided by the processes of addition and subtraction; for him the octave consisted of six tones, and the tone was exactly divisible into fractions such as the half and quarter, so that the fourth was equal to two tones and a half, the fifth to three tones and a half, and so on. On the other hand we have preserved for us in Ptolemy's *Harmonics* the computations of a number of mathematicians, who realized correctly that intervals could only be expressed as ratios (e.g. of string-lengths), that the octave was less than the sum of six whole tones and that this tone could not be divided into equal parts. These authorities are Archytas, the Pythagorean of the early fourth century, Eratosthenes (third century), Didymus (first century) and Ptolemy himself (second century A.D.). To these we must add the scale of Plato's *Timaeus* (35B) and, closely related to it, the computations of the pseudo-Philolaus (*ap. Boethium, Mus. III, 8*) and of Boethius himself (IV, 6).<sup>1</sup> Aristoxenus is less easy to understand than the mathematicians because of the unscientific nature of his postulates. His importance, however, is very great, not only from his comparatively early date but because he claims to champion the direct musical consciousness against the scientific approach of some of his predecessors and contemporaries. But if they are under suspicion of letting irrelevant factors intrude into their calculations, he must equally be suspected of yielding to the attractions of symmetry and convenience. Only their mutual agreement, perhaps, can establish any point strongly. This article is an attempt, first of all, to state what precisely Aristoxenus says directly or by implication about the intervals of Greek music, secondly, to compare his evaluations with the ratios of the mathematicians and so consider what his rough-and-ready mathematics may conceal in the way of real musical intervals.

I. In the fragments of Aristoxenus' writings known to us as the *Harmonic Elements* are preserved two separate but similar accounts of the genera or γένη τῆς μελωδίας (I, pp. 21-27; II, pp. 46-52, Meibom). Like theorists before and after him he considers a typical tetrachord or group of four notes composing the interval of the fourth, of which the extremes (μέση, ὑπάτη) remaining fixed the means (λιχανός, παρῡπάτη) move in each of the three genera (diatonic, chromatic, enharmonic) within a certain locus. The genera are characterized by these loci; the nuances (χρόαι) are special cases of each genus, selected by Aristoxenus on a principle that will be discussed later. These loci he sets out to discover (22, 24), taking first the higher note, λιχανός. The upper limit he finds in the distance of a tone from μέση, appealing to the practice of the diatonic genus.<sup>2</sup> The lower limit is a matter of controversy, in which Aristoxenus' emotions are closely engaged. In his view the λιχανός that is separated by a ditone from μέση was not only a real fact of music but characterized

<sup>1</sup> Gevaert (*Histoire et Théorie I.*, pp. 304-327) was the first to give these formulae the importance that is their due. This article owes much also to the work of P. Tannery (*Mémoires*

*Scientifiques III*, 97-115, reprinted from *Revue des Et. Gr.* XV, 336-352).

<sup>2</sup> 22, 33 οὐχ before ὁμολογεῖται is rightly bracketed by Macran.

its noblest and most beautiful type, as employed in certain ancient styles to which he refers. To his grief this true enharmonic is being banished by contemporary performers, who prefer the higher lichanos of the chromatic, and even when they employ the enharmonic approximate it to the chromatic with an inevitable alteration of character. The implications of this passage (23, 4-22) are of the utmost importance.

The locus of lichanos is thus a tone. That of *παρυπάτη* is the smallest diesis, i.e. a quarter-tone, since it is never less than a quarter-tone or more than a semitone above *ὑπάτη*. The two loci, then, meet at a point a semitone above *ὑπάτη*. Aristoxenus now turns to consider the genera and nuances individually. He defines a *πυκνόν* as a combination of two intervals which together are less than the remaining interval that makes up the fourth. The smallest *pycnon* consists of two smallest enharmonic dieses (he has already—p. 21—defined the smallest enharmonic diesis as a quarter of a tone, the smallest chromatic diesis as a third); next to it comes that which consists of two smallest chromatic dieses. And so on. Aristoxenus establishes the position of lichanos in each of his six nuances. We need not follow the details here, but we may notice certain points. His use of the imperative of definition suggests perhaps something arbitrary. And indeed on his own assumptions it must be so. If lichanos moves within a certain locus, then theoretically it may occupy an infinity of positions (26, 14 *νοητέον γὰρ ἀπείρους τὸν ἀριθμὸν τὰς λιχάνους*). There is not one enharmonic lichanos solely. Aristoxenus' argument is not that the *δίτονος λιχανός* gives the only enharmonic intonation, but that it gives the best.

Secondly, note the great importance given to the note lichanos in establishing the nuance and the relatively small stress laid upon *parhypate*, to which he turns in 26, 29. There are two loci of *parhypate*, one peculiar to the enharmonic, the other shared by the chromatic and diatonic. This for the first time brings us to the question of the combination of lichanos and *parhypate* in a single nuance and the relative size of the three intervals of the typical tetrachord. So far we have only been told that the smallest (enharmonic) *pycnon* consists of two smallest enharmonic dieses, the next smallest (soft chromatic) of two smallest chromatic dieses. Does this imply that the constituent intervals of a *pycnon* are always equal? On p. 27, 2 it is laid down that (1) the lowest interval (*ὑπάτη—παρυπάτη*) is either equal to or less than the middle one (*παρυπάτη—λιχανός*); (2) the middle interval is either equal to, less or greater than the highest (*λιχανός—μέση*). To illustrate this last possibility he appeals to the sharing of *παρυπάται* by chromatic and diatonic; that tetrachordal division also is legitimate that has the lowest chromatic *parhypate* and the highest diatonic lichanos;<sup>1</sup> or, to express it in figures:  $\frac{1}{3} + 1\frac{1}{3} + 1$ . We shall see the importance of this later. This still leaves untouched the question of the division of a *pycnon* proper, except, apparently, in the cases of the enharmonic and lowest chromatic. We must now turn to the parallel passage in Book II.

On the way thither, however, we may note a passage on p. 44, where there is a reference to mixture of the genera. This mention of mixed types (cf. *μυγνυμένων τῶν γενῶν* 7, 3; also Cleonides 9, 30) is yet more evidence that Aristoxenus was prepared to admit considerable variety within the framework of his scheme.

In Book II, 46, 19 Aristoxenus begins with a brief description of the loci of lichanos and *parhypate* in slightly different terms. He then answers the objection that notes bounding different magnitudes should be called by different names. Among other arguments he points out that this would make necessary an infinity of names. It would also be impossible (and this is the point of interest) to decide between rival claimants for the title of e.g. lichanos. *τί μᾶλλον τὴν δίτονον λιχανὸν*

<sup>1</sup> P. 27, 9: the text is corrupt, but Macran's *χρωματικῆς τῆς παρυπάτης* for *χρωματικῆς παρυπάτης* is almost certainly right. Marquard imported the more general phrase used in the correspond-

ing passage in Book II. There is no reason why Aristoxenus should not have been more precise here than there and in III, 73 (*τῆς παρυπάτης ἐπὶ βαρὺν κινήσεως*).

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λεκτέον ἢ τὴν μικρῶ συντονωτέραν; ἁρμονία μὲν γὰρ εἶναι τῇ αἰσθήσει κατ' ἀμφοτέρας τὰς διαιρέσεις φαίνεται, τὰ δὲ μεγέθη τῶν διαστημάτων δῆλον ὅτι οὐ ταῦτ' ἐν ἑκατέρᾳ τῶν διαιρέσεων. Again we have admission of a type of enharmonic whose lichanos was slightly higher than the δίτονος λιχανός that Aristoxenus approved, but still unmistakably enharmonic.

At 50, 15 he redefines πυκνόν. He then proceeds to give those tetrachordal divisions 'which stand out from the rest as familiar (ἐξάρετοί τε καὶ γνώριμοι), because the magnitudes of the intervals in them are familiar' [Macran]. Thus he gives one enharmonic ( $\frac{1}{4} + \frac{1}{4} + 2$ ) and three chromatic, μαλακόν ( $\frac{1}{3} + \frac{1}{3} + 1\frac{1}{3}$ ), ἡμιόλιον ( $\frac{2}{3} + \frac{2}{3} + 1\frac{2}{3}$ ), τονιαῖον ( $\frac{1}{2} + \frac{1}{2} + 1\frac{1}{2}$ ). In the description the emphasis is on lichanos again (that is, on the total size of the pycnon), but here it is definitely stated that these pycna are divided in half by parhypate. Up to this point, he says (51, 11), both notes move; after it parhypate stands still, having run through its locus, while lichanos rises by a quarter-tone; the division ceases to contain a pycnon, and we reach the first diatonic variety, μαλακόν ( $\frac{1}{3} + \frac{2}{3} + 1\frac{1}{3}$ ). One more diatonic remains, σύντονον ( $\frac{1}{2} + 1 + 1$ ). There are then six different λιχανοί, one for each nuance, but only four παρνηπάται, because the last three varieties have the same. Not only so (and this is the same significant addition as before), but all the three higher παρνηπάται are common to diatonic and chromatic alike. Again we have the rules for the relative size of intervals in the tetrachord. But here the possibility that the lowest interval may be smaller than the middle one is illustrated not only from the diatonic but from the following particular case of the chromatic:  $\frac{1}{3} + \frac{2}{3} + 1\frac{1}{3}$ . καὶ γὰρ αἱ τοιαῦται διαιρέσεις τῶν πυκνῶν ἐμμελεῖς φαίνονται.<sup>1</sup> The intervals are still what Aristoxenus would call γνώριμα, but he has acknowledged that his standard types in which parhypate moves proportionately to lichanos are simplifications which do not cover all the possibilities of genuine melody. He goes on to deny that the lowest interval can be greater than the middle one, giving two divisions which he stigmatizes as ἀνάρμοστοι:  $\frac{1}{2} + \frac{1}{2} + 1\frac{1}{2}$  and  $\frac{2}{3} + \frac{7}{24} + 1\frac{5}{6}$ .<sup>2</sup> As an instance of a middle interval greater than the highest Aristoxenus does not this time give the specific case of  $\frac{1}{3} + 1\frac{1}{3} + 1$ , but speaks generally of the combination of the highest diatonic lichanos with a parhypate lower than that which is a semitone above hypate. He excludes presumably the enharmonic parhypate, thus leaving two possibilities:  $\frac{1}{3} + 1\frac{1}{3} + 1$  (as in Book I) and  $\frac{2}{3} + 1\frac{1}{3} + 1$ .

To sum up, we may say that Aristoxenus' primary object is to delimit the spheres of enharmonic, chromatic and diatonic by defining the loci of the movable notes in each (a task, he says, never before attempted in theory: 35, 4); then within each of these to enumerate certain simple and intelligible types. He himself reveals that they do not represent all the genuinely melodious divisions, and in particular that equal division of the pycnon is not obligatory. Far less do they represent all conceivable divisions, which are infinite; and in particular he refers to an enharmonic lichanos lying between his lowest enharmonic and his lowest chromatic as being popular in his day but not accepted by him. Was it partly because he could not reduce the intervals it gave to γνώριμα μεγέθη?

To facilitate comparison with tetrachordal divisions expressed in ratios, I here give the nuances that occur in Aristoxenus' account with the value of their intervals in logarithmic cents (1,200 to the octave) in brackets:<sup>3</sup>

<sup>1</sup> The same possibility of equal or unequal division of a pycnon is expressed in III, 73, 20. In I, 29, 16 he says that the small intervals are equal ὡς ἐπὶ τὸ πολὺ.

<sup>2</sup> The latter is close to the enharmonic of Archytas, as we shall see, and the former not far from the chromatic of Didymus, both of which break this rule laid down by Aristoxenus and repeated by Ptolemy (*Harm.* II, 14). Was this

chromatic in actual use at an early date? And is there a polemical purpose behind the selection of these two instances?

<sup>3</sup> In order to give whole numbers I have allowed inconsistencies involving one cent in the figures for one and a half tones and between the three-quarter tone in the soft diatonic and the three-quarter tone pycnon of the ἡμιόλιον χρώμα.

*Enharmonic*:  $\frac{1}{2} + \frac{1}{4} + 2$  (50 + 50 + 398).

*Chromatic*:

(μαλακόν)  $\frac{1}{3} + \frac{1}{3} + 1\frac{5}{6}$  (66 + 66 + 366).

(ἡμιόλιον)  $\frac{2}{3} + \frac{2}{3} + 1\frac{3}{4}$  (75 + 75 + 348).

(τονιαῖον)  $\frac{1}{2} + \frac{1}{2} + 1\frac{1}{2}$  (100 + 100 + 298).

(mixed, p. 52)  $\frac{1}{3} + \frac{2}{3} + 1\frac{1}{2}$  (66 + 133 + 299).

*Diatonic*:

(μαλακόν)  $\frac{1}{2} + \frac{2}{4} + 1\frac{1}{2}$  (100 + 149 + 249).

(σύντονον)  $\frac{1}{2} + 1 + 1$  (100 + 199 + 199).

*Diatonics with chromatic parhypate*:

$\frac{1}{2} + 1\frac{1}{2} + 1$  (66 + 233 + 199).

$\frac{2}{3} + 1\frac{1}{3} + 1$  (75 + 224 + 199).

II. *Enharmonic*.—Eratosthenes apart, the evaluations of the enharmonic fall into two groups according as they make the pycnon consist of a major semitone ( $\frac{1}{2}$ ) or a leimma ( $\frac{2}{3}$ ). To the former class belong: *Archytas*  $\frac{2}{3} \times \frac{2}{3} \times \frac{5}{4}$  (63 + 49 + 386): *Didymus*  $\frac{2}{3} \times \frac{2}{3} \times \frac{5}{4}$  (55 + 57 + 386): *Ptolemy*  $\frac{4}{8} \times \frac{2}{3} \times \frac{5}{4}$  (38 + 74 + 386). The lichanos here is a major third from μέση, less by a comma ( $\frac{8}{9}$ ) = 22 c.) than the full ditone ( $\frac{9}{8} \times \frac{9}{8} = \frac{81}{64} = 408$  c.). The latter class is, in effect, based upon the diatonic sequence ( $\frac{2}{3} \times \frac{5}{4} \times \frac{9}{8}$ ) which we find in Plato's *Timaeus*. It is the product of λήψις διὰ συμφωνίας, and so to be found in those authors who give us a 'Sectio Canonis.' It has been held to be characteristic of the Pythagorean school, but on inadequate grounds. It certainly owed its later theoretical importance to the influence of Plato; and it is very doubtful if the Pythagoreans of the sixth and fifth centuries did more than establish the harmonic framework of standing-notes with the series of numbers 6, 8, 9, 12.<sup>1</sup> There is reason to believe that the pseudo-Euclidean *Sectio Canonis* is an Academic document, while the enharmonic and chromatic tetrachords of the pseudo-Philolaus (*l.c.*) cannot possibly be Pythagorean, since they ignore the impossibility of equal division of tone and semitone, which must have been recognized by the early Pythagoreans. We need only note in passing that pseudo-Philolaus divides his enharmonic pycnon of a leimma into two equal diaschismata without attempting to evaluate the latter. Boethius (IV, 6) divides the  $\frac{2}{3}$  pycnon into  $\frac{5}{12} \times \frac{5}{12} \times \frac{9}{8}$  by the naïve formula  $\frac{2x}{x+y} \times \frac{x+y}{2y} = \frac{x}{y}$ . The tetrachord of Eratosthenes is close to the ditonal scale with a slight variation:  $\frac{4}{9} \times \frac{3}{8} \times \frac{9}{8}$ .<sup>2</sup>

Before we can consider which of these types Aristoxenus intended to represent by his enharmonic, we must of course be clear that he intended to represent one of them. The term 'equal temperament' is often used in connection with Aristoxenus; and in a sense by dividing the octave into six and the tone into two he has produced 'equal temperament.' But the difference between his procedure and the temperament of modern theory and practice is more important than their resemblance. Our equal temperament is dictated by practical convenience in the matter of modulation. The modern theorist knows that the intervals are distorted upon a tempered instrument and by how much. But Aristoxenus did not live in an age when temperament in the modern sense was either necessary or desirable. The very existence of such variant intonations as he describes would have reduced it to futility. He believed that his semitone was equal to half a tone; he did not say 'I will make a semitone which shall be half a tone, and that tone slightly less than a true tone.' He did not set out to distort slightly his fourths and fifths; but when in the course of experiment difficulties arose he said 'The consonances vary within a minute locus.' Four

<sup>1</sup> It may be, as Tannery suggests, that confronted with the incommensurability of the tone and varying musical practice they relegated the movable notes to the realm of ἀπειρον and refused to speculate upon them, till Archytas tackled the problem on more realistic lines. Aristoxenus himself, who in Books I and II selected certain magnitudes as γνῶριμα from the infinite possibilities, in Book III, 69, 6 affirms that in these matters of pitch and magnitude scientific treatment is impossible, since the possibilities are

unlimited; the functions and mutual arrangements of notes in the scale only are πεπερασμένα.

<sup>2</sup> The ratios of Eratosthenes' enharmonic are dictated by his choice of the minor third ( $\frac{5}{8}$ ) for the upper interval of his chromatic and by the assumption, made also by Didymus and Boethius, that the pycnon of the enharmonic should be equal to the lowest interval of the chromatic. Dividing his chromatic pycnon  $\frac{1}{2}$  into  $\frac{2}{3} \times \frac{2}{3}$ , he then takes  $\frac{2}{3}$  as his enharmonic pycnon, leaving  $\frac{1}{3}$  for the highest interval.

passages throw light on his point of view. Two are from the first book, two from the second; and they seem to show a development. On p. 24 he shelves the question of the commensurability of the intervals making up the fourth, but decides to assume that it consists of two and a half tones (*ὡς φαινόμενον ἐκείνου δύο τόνων καὶ ἡμίσεος*). The guilty conscience appears again on p. 28. The ditone, he says, is either eight times the smallest diesis or very slightly less. Between the writing of the two versions represented by Books I and II he seems to have devised an experiment, if it can be dignified by such a name, proving to his satisfaction that the fourth consists of two and a half tones. It is described on p. 56. We need not examine it in detail. Its success depends on a slight distortion of the fifths or fourths or both by means of which it is conducted; if these are all true the interval finally obtained will be in error by 24 cents. Its success, that is to say, in theory; for in practice a strictly accurate result was unlikely. Is it not a sufficient commentary upon it that, apart from the initial fourth and the supposed resulting fifth, it involves the judging by ear of ten successive consonances? If he had once after an accumulation of small errors obtained a recognizable fifth, would not that have satisfied him? How often did he conduct this operation? If it did not always come out right, it may have been this that led him in the fourth of our passages (p. 55) to say that even the magnitudes of the consonant intervals perhaps vary within an extremely minute locus (*τόπον . . . παντελῶς ἀκαριαῖόν τινα*). This is a very vague kind of 'temperament'!

Which, then, of the enharmonic types does Aristoxenus intend to represent by his enharmonic? His ditone of approximately 398 cents falls between the major third (386 c.) and the true ditone (408 c.). It is slightly closer to the latter, which I believe it to represent. Since Aristoxenus believed that his tone, ditone and semitone could be obtained by means of the consonances, there is surely a presumption that they are respectively  $\frac{9}{8}$ ,  $\frac{3}{2}$  and  $\frac{4}{3}$ . It needs a distortion of consonances to obtain 'equal temperament' by this method, but a still greater one to obtain the major third and semitone. There is a further reason. As we have seen in the earlier section, Aristoxenus more than once mentions a certain type of enharmonic as popular in his day. It had a slightly higher lichanos than the type he preferred, but was still genuinely enharmonic; that is to say, its highest interval was greater than that of his own lowest chromatic, greater than 366 cents. It is hard not to believe that his own *δίτονος λιχανός* is that which gives an upper interval of a strict ditone, while that which is *μικρῷ συντονωτέρα* gives the major third of Archytas' enharmonic. If that is so, Aristoxenus recognizes here, at least, the difference of a comma between the ditone and the major third. And this is the difference between a major and a minor tone. The further implications of this must wait till we consider the diatonic.

But if the ditone is really a ditone, then the semitone is really a leimma (90 c.), and less than an equal semitone, though nearer to it than is the major semitone (112 c.) on the other side. As a rough approximation this is as good as we can expect from our author. But we must here face a piece of evidence that tells against the interpretation that has been adopted. In a passage already noted (p. 28) Aristoxenus grudgingly admits that the ditone may be slightly less than eight times the enharmonic quarter-tone. That is, the semitone is slightly more, not slightly less, than the equal semitone. The passage would seem to point to the lichanos of Archytas. In that case, it is hard to conceive what the *μικρῷ συντονωτέρα λιχανός* can be. I think we can understand the working of Aristoxenus' mind which produced this statement without abandoning our first hypothesis. How did he arrive at the conclusion that the ditone might be less than eight quarter-tones? Not by adding quarter-tones together till he found out, certainly. It is a concession to mathematical doctrine in some form. It might, it is true, be an admission that the  $\frac{3}{4}$  interval of Archytas was less than four times  $\frac{1}{8}$ , if that fact had ever been



brought to Aristoxenus' notice. But it might also be due to a much simpler form of objection. It had certainly been established by mathematicians that the fourth was less than two whole tones and a half. Now Aristoxenus believed, first on faith, later on the evidence of a bad experiment, that his semitone was a correct half-tone. If any concession was to be made it should be made in the other direction: the ditone should be less than two full tones rather than the semitone less than half a tone.

Apart from this passage there is an objection of a more general kind that might be made to the view advanced. Aristoxenus claims to trust his ear and to represent the facts of practical music. If the ditonal scale was a theoretical elaboration of the Academy, it is unlikely that Aristoxenus had its intervals in mind for his enharmonic. Further, to our ears the scale of Archytas with its major third would appear far more melodious than the Platonic scale with its harsh third.<sup>1</sup> The answer is suggested by various considerations. Aristoxenus' *δίτονος λιχανός* is clearly a lost cause. The enharmonic in any form is rarely attempted and when it is the *δίτονος λιχανός* is slightly raised. The process is described as one of 'sweetening' (*γλυκαίνειν*). What is this but the employment of the major third instead of the harsher ditone? Secondly, though the ditonal scale may owe its theoretical importance to Plato's *Timaeus*, it is absurd to suppose it was invented by him. Immediately the ratio expressing the tone ( $\frac{9}{8}$ ) had been discovered it was surely likely that theorists, not necessarily Pythagoreans, would try to evaluate the tetrachord by subtracting the tone twice from the interval of the fourth. By the remainder they were baffled, and it is quite possible that the speculations (and terminology) found in the pseudo-Philolaus are pre-Platonic. But, more important still, a scale of this sort is the direct result of tuning a stringed instrument by means of the consonances of the fifth and fourth. Starting from *μέση* A a fourth and a fifth give the two E's, a fourth upward D; a fifth downward from D gives G, a fourth upward from G C; thence a fifth downward F, and we have a diatonic scale consisting of major tones and leimmata. The effect must have been familiar, even if the tuning was subsequently adjusted to produce sweeter thirds. Indeed, Aristoxenus may be right in holding that this was the actual intonation of the older and severer style of music. That it did not entirely disappear is testified by Ptolemy, where he describes (*Harm.* I, 16; II, 16) the intonations most in use in his day for the lyre and cithara.<sup>2</sup>

As to the division of the pycnon, practice, at least in his day, may have approximated to Aristoxenus' version by aiming at strict equality. Eratosthenes and Didymus both give approximate equality in the pycnon, and the slight difference between the two ratios is really a mathematical fiction. Ptolemy's lower interval is little more than half the upper one and less than a fifth of a tone.<sup>3</sup> Both he and Aristoxenus declare that the lower should not be the greater; and, in so far as the parhypate was a leading-note subject to attraction by hypate (as the leading-note of our major scale is subject to attraction *upwards* towards the tonic), the principle seems reasonable. But we know so little of the real nature of Greek melody that we cannot sum-

<sup>1</sup> The tempered thirds we tolerate are actually nearer to Plato than to Archytas.

<sup>2</sup> Note that it is the practical musicians, not any theoretical *καθηγορικοί*, to whom it is ascribed. They tended, he says, to substitute it for the intonation  $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2}$ . And, as it was used in combination with a tetrachord of  $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2}$ , we can see clearly one reason why they did so:  $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2}$  in the E mode gives two false fourths and two false fifths; the substitution of  $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2}$  in the upper tetrachord gives all true fourths and only one false fifth [F-C]. It is also interesting to note that these intona-

tions of the practical musicians of Ptolemy's time show a complete avoidance of both major and minor thirds, except in the tetrachord for which the ditonal type was substituted!

<sup>3</sup> Tannery suggests that this is the legacy of some earlier theorist who wished to deny that Aristoxenus' quarter-tone was the smallest melodious interval. It seems to me more likely that he adopts here for the sake of uniformity the principle of division by tripling the terms ( $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4} \times \frac{1}{4} = \frac{1}{8} \times \frac{1}{8}$ ) which gave him most satisfactory results for his chromatics. The enharmonic was extinct in his day.



marily dismiss the  $\frac{25}{27}$  interval that Archytas places in the lower position, and we shall later see reason to relate it to other information we possess.

*Diatonic.*—As the chromatic nuances of Aristoxenus present points of particular difficulty we will pass straight to the diatonic. If our interpretation of the enharmonic is correct it carries important implications for the diatonic. For us this enharmonic has in the strict sense the  $\delta\acute{\iota}\tau\omicron\nu\omicron\varsigma$   $\lambda\acute{\iota}\chi\alpha\nu\acute{o}\varsigma$ . A comma higher comes the lichanos of Archytas' enharmonic. To this Aristoxenus refers, but does not take it as one of his types, partly perhaps because he disliked the school of Archytas (he was, by all accounts, a maliciously-minded person), more probably because he could not reduce it to such fractions as seemed to him intelligible. The smaller intervals of his system are the quarter- and third- tones; the interval of three-eighths of a tone only enters it *quâ* one and a half times the quarter-tone, and even in that nuance the lichanos, the important note, is three-quarters of a tone above  $\acute{\iota}\pi\acute{\alpha}\tau\eta$ . He might have represented the pycnon of Archytas by seven-twelfths of a tone, but he would not have considered it  $\gamma\nu\acute{\omega}\rho\mu\omicron\nu$ . However, that he did distinguish his own enharmonic from that of Archytas shows that he was aware of the comma difference, and makes it therefore unlikely that he would fail to distinguish a major from a minor tone if he wished to represent a diatonic scale which combined the two. Actually the varieties of diatonic in Aristoxenus are as follows:

- (1)  $\frac{1}{2} + 1 + 1$  (100 + 199 + 199). (2)  $\frac{1}{2} + \frac{2}{3} + 1\frac{1}{4}$  (100 + 149 + 249).  
 (3)  $\frac{1}{3} + 1\frac{1}{3} + 1$  (66 + 233 + 199). [And perhaps (4)  $\frac{2}{3} + 1\frac{1}{3} + 1$  (75 + 224 + 199).]

Let us set against them the evaluations of the mathematical theorists. (a) First we have the scale of the *Timaeus* and the  $\kappa\alpha\nu\omicron\nu\iota\kappa\omicron\iota$ , which is also that of Eratosthenes and Boethius and employed under certain circumstances by the professional string-players of Ptolemy's day:  $\frac{25}{27} \times \frac{6}{5} \times \frac{9}{8} \times \frac{9}{8}$  (90 + 204 + 204). (b) Didymus and Ptolemy ( $\sigma\acute{\upsilon}\nu\tau\omicron\nu\omicron\nu$ ) both give us a diatonic consisting of major and minor tones and a major semitone. Ptolemy's is as follows:  $\frac{1}{2} \times \frac{9}{8} \times \frac{1}{2} \times \frac{1}{2}$  (112 + 204 + 182); Didymus has the tones in reverse order. (c) Archytas and Ptolemy ( $\tau\omicron\nu\nu\iota\alpha\acute{\iota}\omicron\nu$ ) both have a diatonic containing a septimal tone:  $\frac{25}{27} \times \frac{9}{8} \times \frac{9}{8}$  (63 + 231 + 204). (d), (e) Ptolemy gives us two further varieties, a soft diatonic:  $\frac{25}{27} \times \frac{1}{2} \times \frac{9}{8}$  (85 + 182 + 231) and the curious  $\delta\mu\alpha\lambda\acute{o}\nu$ :  $\frac{1}{11} \times \frac{1}{10} \times \frac{1}{9}$  (151 + 165 + 182). Now if we take Aristoxenus' enharmonic as equivalent to a pycnon of  $\frac{25}{27}$  plus a strict ditone, we are bound also to take his diatonic (1) as the equivalent of the scale of the *Timaeus*; for he expressly states that the lichanos of the one is the parhypate of the other. Where then is that variety in the size of the tones that we find in the other computations? Above all, where is the minor tone ( $\frac{1}{9}$ ) that we find in our own 'just' intonation? If nowhere else, do we not find here a kind of 'temperament' in Aristoxenus, by which a perfect fourth consists of a major semitone and two equal 'tempered' tones? This is the view of Tannery (*l.c.*), and it is superficially attractive. But it is inconsistent even with such evidence as we have in favour of 'temperament' in Aristoxenus. It is inconsistent with his view that the tone is a magnitude that can be obtained by subtracting the fourth from the fifth. Tone, ditone and semitone are all intervals which he believed could be found by the process of  $\lambda\acute{\eta}\psi\iota\varsigma$   $\delta\acute{\iota}\alpha$   $\sigma\omicron\mu\mu\acute{\epsilon}\omega\nu\iota\alpha\varsigma$ , so that any tempering that was contemplated must also apply to the consonances. The most that could be said is that he failed to distinguish, or turned a blind eye upon, the comma difference between major and minor tones. The likelihood of this depends upon the general degree of accuracy shown in all his computations; and judgement should thus be suspended until we have them all under review. But at least the absence of variety of tones cannot be adduced as evidence, unless the nature of Aristoxenus' system is misunderstood. His nuances are not exclusive but representative types. Quite apart from the soft diatonic (2), of which consideration must be postponed, he does in effect admit a variety of diatonic containing unequal tones.

The intervals of his diatonic (3) are, expressed in cents, 66+233+199. Those of Archytas' diatonic (c) are 63+231+204. The discrepancy is minute. It is surely impossible to doubt that the diatonic Aristoxenus had in mind was that of Archytas, and that it was in general practical use. Any doubt of its practicality would be set at rest by the discovery that Ptolemy not only records it but regards it as the most fundamental type of diatonic, and declares that the artists of his day only used the other intonations in combination with it, a tetrachord of each. This, then, is a most interesting fact about Greek music that alike in the fourth century B.C. and the second century A.D. the Greeks used a diatonic scale containing septimal tones (§).

There remains unmentioned by Aristoxenus the type (b), our 'just' diatonic, the σύντονον of Ptolemy, with the variation of it that occurs in Didymus. If Aristoxenus could speak of an enharmonic lichanos which, compared with the δίτονος λιχανός, was μικρῶ συντονωτέρα, he could equally well have spoken of a diatonic παρῆπατή that was similarly slightly raised. He does not, and various causes may have led him not to do so. As in the case of the enharmonic, the resulting intervals cannot be expressed in the fractions he favours; and there was no necessity for him to mention varieties to him anomalous, unless he had either a point to make as in the case of diatonic (3) or a grievance to air as in the case of the enharmonic. But perhaps it weighed more with him that such an admission would have disorganized his theory of the loci of lichanos and parhypate. To admit a lichanos higher than the lowest lichanos was simple, but to have a parhypate wandering by however small an interval into the locus reserved for λιχανοί would never do. The loci might meet at a point but not overlap. Thus, if he was aware of a kind of diatonic in which this took place, he has suppressed it—or else it is impossible to extract any consistent sense out of his doctrine. But it is not certain that our normal diatonic (or that of Didymus) was in common use at that time, and it may well have been that when the citharodes had turned their instruments to the ditonal scale the intonation to which they adjusted them, if at all, was that which was obtained by slackening the παρῆπατοιειδῆ only to obtain a septimal tone from the λιχανοειδῆ. The evidence of Archytas is strong. We must now see what bearing it has upon the even harder problems of the chromatic nuances.

*Chromatic.*—Aristoxenus gives three types: (a) μαλακόν  $\frac{1}{3} + \frac{1}{3} + 1\frac{5}{8}$  (66+66+366), (b) ἡμιόλιον  $\frac{2}{3} + \frac{2}{3} + 1\frac{3}{4}$  (75+75+348), (c) τριαίον  $\frac{1}{2} + \frac{1}{2} + 1\frac{1}{2}$  (100+100+298); and we may here consider also (d) the soft diatonic:  $\frac{1}{2} + \frac{3}{4} + 1\frac{1}{4}$  (100+149+249). The mathematical theorists give us two types in which the pycnon is a minor and a major tone respectively. Into the first class fall: (i) Eratosthenes— $\frac{2}{3} \times \frac{1}{2} \times \frac{1}{2}$  (89+93+316); (ii) Didymus— $1\frac{5}{8} \times \frac{2}{3} \times \frac{2}{3}$  (112+70+316); (iii) Ptolemy (μαλακόν)— $\frac{2}{3} \times \frac{1}{2} \times \frac{1}{2}$  (63+119+316). Into the second fall those κανονικοί who made the pycnon consist of leimma+apotome, the latter being the difference between the tone and the leimma (cf. Gaudentius, p. 343, Jan), the pseudo-Philolaus, who split the difference between leimma and apotome without attempting a mathematical evaluation, and (iv) Archytas:  $\frac{2}{3} \times \frac{2}{3} \times \frac{2}{3}$  (63+141+294).

Let us consider Aristoxenus' (c) first. Does its pycnon of a tone represent the 182 cents of Eratosthenes or the 204 cents of Archytas? The difference is less than a comma in either case, but it is definitely nearer to that of Archytas. Further, the fact that Archytas prefers the above formula with clumsy ratios that are not of the favoured  $\frac{n+1}{n}$  type to the simpler  $\frac{2}{3} \times \frac{1}{2} \times \frac{1}{2}$  would seem to indicate that the major-tonal pycnon was favoured in contemporary practice. But Aristoxenus differs from Archytas in the division of the pycnon by giving it equal intervals.<sup>1</sup> His parhypate is that

<sup>1</sup> The nearest approach to these equal semi-tones in mathematical dress is to be found in

Aristides Quintilianus, p. 117, Meibom, who divides the tone as follows:  $\frac{2}{3} = 1\frac{1}{3} \times 1\frac{1}{3}$ .

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of the diatonic, so that, if that gave a leimma to hypate, it gave an apotome to lichanos here. The difference is the 'Pythagorean comma' of 24 cents. It seems likely that this difference between two consecutive small intervals was neglected by Aristoxenus, while he was aware of a slightly smaller difference in two rival tunings of the same string, comparison being obviously more difficult in the former case. This is the nearest to equal division of the major tone that makes musical sense.<sup>1</sup> But we must also call to mind that Aristoxenus regarded all the *παρνπάται* above the enharmonic as common to all chromatic and diatonic *λιχανοί*, and gave as an example of a melodically satisfactory combination: (e)  $\frac{1}{3} + \frac{2}{3} + 1\frac{1}{3}$  ( $66 + 133 + 299$ ). Compare this with Archytas' chromatic:  $63 + 141 + 294$ . The maximum difference is one of 8 cents, that is, practically negligible. Just, then, as we discovered Archytas' diatonic in Aristoxenus' scheme, so now we have also found his chromatic. This agreement is surely very significant. We can hardly avoid the conclusion that in the *τονιαῖον χρώμα* also Aristoxenus' tone is the major tone. Before we pass on to the chromatics of lower lichanos, we may note that none of Aristoxenus' evaluations corresponds closely to the chromatics of Eratosthenes, Didymus and Ptolemy (soft). I find it easier to believe that this reflects the practice of his time than that he either failed to recognize the distinction between major and minor tones or deliberately adopted a compromise between them.

There remain the two lower chromatics, (a) *μαλακόν* and (b) *ἡμιόλιον*, and the soft diatonic. I cannot pretend to solve the problems that arise when we try to interpret them in terms of musical practice. I have, however, various suggestions to make. The first point that is remarkable is the extraordinary closeness between the two chromatics, which Aristoxenus yet thought it worth while to record as distinct types. Even between the respective *λιχανοί* of (a) and (b) there is only a difference of 18 cents, less than a comma; that between the *παρνπάται* is only half as much. We must, however, keep in the forefront of our minds that Aristoxenus was not wedded to equal division of the pycnon in the chromatic but regarded the various *παρνπάται* as common; we must then examine all the values involved independently as well as in combination.

First we should consider a theory advanced by Tannery in an article published posthumously ('Sur le spondiasme dans l'ancienne musique grecque,' *Rev. archéol.*, 1911, Vol. I, pp. 41-50 = *Mém. Scient.* III, 299-309). There he attempts to identify the soft chromatic lichanos with that of Archytas' enharmonic and the hemiolic lichanos with that of the chromatic of Eratosthenes. Thus two-thirds of a tone represents  $\frac{1}{3}$ , three-quarters of a tone represents the minor tone  $\frac{1}{4}$ ; and it is pointed out that the minor-tonal pycnon of the hemiolic type is very nearly one and a half times the major semitone  $\frac{1}{2}$ . There are then, in effect, only four upper intervals in enharmonic and chromatic nuances ( $\frac{81}{64}$ ,  $\frac{5}{4}$ ,  $\frac{6}{5}$ ,  $\frac{32}{27}$ ), and the four types of Aristoxenus are inaccurate interpretations of them. The conclusion would be a satisfying one if we could feel it was honestly come by. But there are objections. In the first place the hemiolic lichanos of Aristoxenus is one and a half times not his soft chromatic but his enharmonic lichanos; 182 cents is near enough to being one and a half times 112 cents; but in the case of 182 and 90 the error is too great. That is to say, Tannery's interpretations of the two chromatics are mutually inconsistent. There are also difficulties about each taken separately. The equation of two-thirds of a tone with  $\frac{1}{3}$  involves an error of about a comma; it therefore just doubles the true difference between  $\frac{81}{64}$  and  $\frac{1}{3}$ . The equation of three-quarters of a tone with  $\frac{1}{4}$  involves the still larger error of 32 cents. Further, whatever three-quarters of a tone means here, it presumably means the same also in the soft diatonic, where it occurs as the middle

<sup>1</sup> Similarly, the minor tone can be divided with approximate equality, as by Eratosthenes; but

it is the division of Didymus that gives the more convincing intervals.



interval. The mathematicians give us two evaluations with a minor tone in this position—Didymus' diatonic:  $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2}$  (112+182+204) and Ptolemy's soft diatonic:  $\frac{2}{3} \times \frac{1}{2} \times \frac{1}{2}$  (85+182+231). Aristoxenus' 100+149+249 can scarcely be intended for the former, since 100 cents absurdly misrepresents the difference between the two tones.<sup>1</sup> The latter is more plausible, granted the error of 33 cents; but we can find a closer parallel than this to Aristoxenus' soft diatonic in Ptolemy.

Let us return to the soft chromatic of Aristoxenus. Its parhypate is a third-tone above *ἡπάτη*, its lichanos two-thirds of a tone. But with the interval of a third-tone we are already familiar and have seen reason to equate it with the ratio of  $\frac{2}{3}$  occurring in Archytas' chromatic and diatonic. This would seem to be a fixed point. But if we have found parhypate, we are far from finding lichanos. The nearest interval to two-thirds of a tone that we can find among the mathematicians is Archytas'  $\frac{2}{3}$ . But this gives us an incredible top interval for the tetrachord, consisting of the combination of a minor tone, a major semitone and Archytas' third-tone. The major third is 20 cents larger, but must belong either to Aristoxenus' enharmonic or to that variant of it that is expressly distinguished from the chromatic. There is in fact no musically probable interval that can be held to be represented by the top interval of this tetrachord and yet distinguished from that of the hemiolic chromatic. I suggest that Aristoxenus, favouring the equal division of pycna, and knowing his third-tone ( $\frac{2}{3}$ ) to be a true musical interval, assumed that by doubling it he could obtain a satisfactory lichanos, and so produced a completely factitious nuance.

The hemiolic chromatic and soft diatonic alike contain the interval of three-quarters of a tone, the former as pycnon, the latter as middle interval. The interval is also known to us as *σπονδειασμός*, both from pseudo-Plutarch *de mus.* § 112 (Weil and Reinach) and from Aristides Quintilianus, p. 28 (Meibom). It was employed apparently in the old Spondeion scale for the undivided 'pycnon' in the upper (and presumably also in the lower) tetrachord, instead of the semitone of the enharmonic pycnon or lowest diatonic interval. Tannery, as we have seen, interprets this three-quarter tone as the minor tone. But there is a musical interval occurring in Ptolemy's tetrachords that is far closer in value. Three-quarters of a tone is worth 148 cents, and the interval of  $\frac{3}{4}$  is worth 151. The latter occurs as the lowest interval of Ptolemy's *ἑμάλιν διὰ τονον*. Whatever may be the truth about this peculiar-looking tetrachord ( $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2}$ ), it is unlikely to have been pure invention of Ptolemy, and it would seem to have relation to the Spondeion. If the 'lichanos' of the Spondeion was  $\frac{3}{4}$  above the hypate,<sup>2</sup> did it remain so when in the course of development the pycnon was divided? If so, into what intervals was it divided? These questions cannot be answered with certainty.  $\frac{2}{3} \times \frac{3}{4}$  would merely give the familiar fiction of equal division. The musical interval nearest in value to Aristoxenus' three-eighths of a tone (75 cents) is our own minor semitone  $\frac{2}{3}$  (70 cents), which occurs as the middle interval of Didymus' chromatic, but it will not conveniently combine in a tetrachord with an upper interval of  $\frac{1}{2}$ . Perhaps we must here, as in the soft chromatic, consider the validity of parhypate and lichanos separately, and regard the intervals of a minor semitone and an undecimal three-quarter tone as both familiar to Aristoxenus from actual practice, but their combination in a single nuance as factitious.

<sup>1</sup> If Aristoxenus wished to represent this type of diatonic and was taking  $\frac{1}{2}$  as three-quarters of a tone, he was faced by a dilemma. Either he must grotesquely exaggerate the difference of the tones, as above, or, representing the major tone, as usual, by his own whole tone, make the lowest interval also three-quarters of a tone, which is absurd.

<sup>2</sup> The interval of  $\frac{3}{4}$  will not really square

with the account in Plutarch, which demands the possibility of confusion between the upper interval of the lower tetrachord (F-A) and the combination of disjunctive tone and *σπονδειασμός* (A-C).  $\frac{1}{2}$  as *σπονδειασμός* makes these intervals practically identical. On this point, and on the Spondeion in general, I would refer the reader to my article on 'The Spondeion Scale' in *C.Q.*, Vol. XXII, 1928.

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This same interval of the three-quarter tone occurs also in the soft diatonic of Aristoxenus. This tetrachordal division assumes the possibility of dividing the fourth into two equal parts, each  $1\frac{1}{2}$  tones in size. The division of the fourth ( $\frac{4}{3}$ ) into simple ratios which comes nearest to this is  $\frac{5}{7} \times \frac{7}{8}$  ( $231 + 267$ ). Aristoxenus'  $1\frac{1}{2}$  tones (249) lie exactly half-way between these two ratios. But it would be somewhat in favour of equating the upper interval of his soft diatonic with  $\frac{7}{8}$  that elsewhere he appears to represent Archytas'  $\frac{5}{7}$  by the interval of  $1\frac{1}{2}$  tones. On the basis of the above division Ptolemy constructs two tetrachords, his soft diatonic  $\frac{2}{3} \times \frac{3}{4} \times \frac{4}{5} \times \frac{5}{6}$  ( $85 + 182 + 231$ ) and his σύντονον χρώμα  $\frac{2}{3} \times \frac{3}{4} \times \frac{4}{5} \times \frac{5}{6}$  ( $80 + 151 + 267$ ). If we compare Aristoxenus' soft diatonic with these we find that in the former case, which sets three-quarters of a tone against the minor tone (as on Tannery's hypothesis), the maximum error is the large one of 33 cents, in the latter it is less than a comma; the middle intervals are practically identical, the semitone, which in the enharmonic represented 90 cents, is here 80, the upper interval of  $1\frac{1}{2}$  tones represents the septimal third  $\frac{7}{8}$ . This last interval shows a divergence of 18 cents, yet I hope to show that it is really the strongest possible reason for equating Aristoxenus' soft diatonic with Ptolemy's σύντονον χρώμα.<sup>1</sup> For the part played by this septimal minor third in Greek music has not yet been fully recognized.

The soft diatonic nuance of Aristoxenus is not the only place in Greek musical theory where the interval of  $1\frac{1}{2}$  tones is mentioned. Two theorists (both perhaps depending ultimately on Aristoxenus) mention the term ἐκβολή and define it as a rising interval of five dieses, or quarter-tones. These are Aristides Quintilianus and Bacchius. It is also mentioned, without definition, in pseudo-Plutarch, *de musica*.

(a) Aristides Quintilianus inserts between his accounts of Modulation and Melodic Composition (p. 28) a brief description of three intervals, namely: ἐκλυσis, a fall of three dieses; σπονδειασμός, a rise of three dieses; and ἐκβολή, a rise of five dieses. These intervals had to be employed, he says, by the ancients πρὸς τὰς διαφορὰς τῶν ἁρμονιών. They were called πάθη τῶν διαστημάτων (whatever that may mean) owing to the rarity of their employment. They are, then, connected in some way with the old ἁρμονίαι.

(b) Bacchius §§ 41, 42 defines ἐκλυσis and ἐκβολή similarly, using the phrase ἀπό τινος φθόγγου ἁρμονίας, where ἁρμονία might perhaps mean 'scale,' though in the only other place where the word occurs in Bacchius it means 'enharmonic.' (That he illustrates the intervals from that part of the scale in which the tetrachords συνημμένων and διεξευγμένων overlap is probably because this offered the only opportunity of illustrating the interval of three dieses.) In an earlier passage, however (§§ 36, 37), he definitely associates them with the enharmonic. After distinguishing between 'standing' and 'movable' notes, he adds to the definition of the latter class the remark 'δι' ὧν τὰ διαστήματα πάντα ἀνίσταται καὶ ἐπιτείνεται πλὴν δύο. The catechism continues. 'Which are these?' 'ἐκλυσis and ἐκβολή.' 'How is this?' 'ἐκλυσis is a fall in pitch, ἐκβολή a rise in pitch.' 'In what genus?' 'In the enharmonic and in no other.'

(c) The pseudo-Plutarch passage (§ 287) associates ἐκβολή and ἐκλυσis with Polymnestus and may ascribe to him their 'invention' (the reading is doubtful).<sup>2</sup>

<sup>1</sup> If this equation is correct, we have the interesting fact that the only type of chromatic that Ptolemy found in practical use in his time was actually regarded by Aristoxenus as diatonic!

<sup>2</sup> καὶ τὴν ἐκλυσιν καὶ τὴν ἐκβολὴν πολὺ μείζω πεποιθέναι φασὶν αὐτόν. πολὺ μείζω certainly makes bad sense, and Reinach brackets it as concealing a marginal Πολύμνηστον or Πολύμνιστον and translates πεποιθέναι by 'il créa.' But εὐρίσκω (or some compound) is usual in

pseudo-Plutarch of musical 'inventions,' and I prefer Westphal's hypothesis of a lacuna after ἐκβολὴν; these accusatives then are constructed with Πολύμνηστον . . . ἀνατιθέασι in the preceding phrase. It is conceivable that, just as Terpanther is associated with the employment in melody of Dorian νῆτη, so Polymnestus popularized the addition of D below an enharmonic E octave. See below.

It must be confessed that our authorities are unsatisfactory. Bacchius is very incoherent. Can any intervals except the fourths and fifths of the harmonic framework (E-A-B-E) and the disjunctive tone be said to be independent of the movements of the 'movable' notes? But it seems that he is trying to distinguish these intervals from the normal mutations within the tetrachord. I believe that the clue to *ἐκβολή* at least is to be found in the disjunctive tone and also in the tone between *ὑπάτη* and the note a tone below *ὑπάτη* (Aristides and Theo Smyrnaeus give us the convenient term *ὑπερυπάτη*). This interval is found combined with an enharmonic pycnon in the old *ἁρμονίαι* described by Aristides (p. 21) and also in the Orestes fragment.<sup>1</sup> There we twice find an instance of the progression P Φ, namely from enharmonic *παρυπάτη* to diatonic *λιχανὸς ὑπατῶν* (or *ὑπερυπάτη*)—an interval, that is, of five dieses. These tones, both, it will be noted, outside the tetrachordal variation, offer no clue to intervals of three dieses; and *σπονδειασμός* must certainly be associated, on the strength of pseudo-Plutarch's evidence, with the lower interval of the Spondeion trichord. It will have been remarked that the intervals quoted from the Orestes fragment are falls, not rises, of five dieses. Why is the term *σπονδειασμός* limited to a *rise* of three dieses? If *ἐκλυσίς* is to be taken as its corresponding fall, it is thereby divorced from *ἐκβολή*. Was there no name for a fall of five dieses? It is hard to believe that the account of these intervals has come down to us aright. It seems to me at least credible that there has been a confusion here and that *σπονδειασμός* meant a rise or fall of three dieses and was associated with the Spondeion scale, while *ἐκλυσίς* and *ἐκβολή* were respectively a fall and rise of five dieses.

However this may be, there is evidence (including that of an actual musical document) for an interval of five dieses, called into being through the relation of the enharmonic pycnon and a tone (unaffected by the genera) lying immediately below it. Now, the employment of such an interval may or may not have been rare, as Aristides says; but in any case it is likely that it was some comparatively simple musical interval and not the result of a haphazard approximate splitting of a semitone. Indeed such an interpretation of the enharmonic pycnon is only possible if we imagine the *μεσόπυκνον* to have been employed merely in relation to the extremes as a 'Durchschleifen durch das Intervall,' as Westphal puts it. It is one of the great values of the Orestes fragment for us that it shows us that this was not so. Now, there are only two intervals smaller than the major semitone ( $\frac{1}{2}$ ) which will make with the tone below a satisfactory interval. One is  $\frac{9}{8}$ , which makes with the major tone ( $\frac{9}{8}$ ) the septimal tone ( $\frac{8}{7}$ ). But this is smaller (27 cents) than any interval which we can postulate for Greek music.<sup>2</sup> The other is  $\frac{2}{3}$ , which with the tone makes the septimal third  $\frac{7}{6}$ .

Perhaps we can now see why Archytas selected  $\frac{2}{3}$  to give the common parhypate of his three genera. As Tannery points out, to understand his ratios it is necessary to consider not only the typical tetrachord (E-A) but also the tone below (D-E). For he obtained his enharmonic lichanos by dividing the fifth (D-A) into

<sup>1</sup> It is found also in the second Delphic Hymn, but there the pycnon is more probably chromatic.

<sup>2</sup> This interval is the difference between the septimal tone ( $\frac{8}{7}$ ) and the major tone ( $\frac{9}{8}$ ). It plays an important part in the theories of Dr. W. Perrett (*Some Questions of Musical Theory*, 1926 and 1928). Though I find difficulty in accepting them in detail, I believe with him that the Greeks used intervals strange to us with precision. They can scarcely, however, have used so small an interval as 27 cents. It is possible that the aulos-player, who could control

the intonation at will, varied the pitch of the enharmonic parhypate according as it was employed in relation to hypate or to hyperhypate. If then in the course of the same piece he made the interval between hyperhypate and parhypate that of  $\frac{7}{6}$  and also employed the lichanos of Aristoxenus, a leimma above hypate, he would in effect be employing two notes distant only by  $\frac{1}{12}$ . Procedure of this sort might also have led Aristoxenus to evaluate  $\frac{7}{6}$  ( $= \frac{8}{7} \times \frac{9}{8}$ ) as  $1\frac{1}{2}$  tones (see p. 207), which an interval of  $\frac{8}{7}$  ( $= \frac{8}{7} \times \frac{9}{8}$ ) between hyperhypate and parhypate would not do, as that he elsewhere equates with  $1\frac{1}{2}$  tones.

$\frac{6}{5} \times \frac{5}{4}$ ; and, having fixed his diatonic lichanos a major tone from A, he divides the fourth (D-G) into  $\frac{7}{8} \times \frac{8}{7}$  and so finds his common parhypate a septimal third above D and the interval of  $\frac{8}{5} \times \frac{5}{4}$  above E. This was no mere mathematical trick. If we are to believe the evidence, he was making a genuine attempt to interpret the actual facts of music, and the criticisms of Ptolemy arise in part from Ptolemy's ignorance. Archytas is in a fair way towards being justified in every department. His diatonic and his chromatic are found to square with tetrachordal divisions admitted melodiously by Aristoxenus. His enharmonic lichanos appears to be that higher enharmonic lichanos that so roused Aristoxenus' ire, while now his parhypate is seen to give with the *ὑπερπάτη* (or, in the upper tetrachord, with *μέση*), an interval which may have been of great importance in the genuine enharmonic music of ancient Greece (an interval which, it may be added, is produced also by his diatonic, which is also Ptolemy's staple diatonic; it is thus the only type of third which occurs freely in the lyre and cithara scales of Ptolemy). Finally, we may add that what is Archytas' justification is also perhaps the explanation of a peculiarity of the notations that has often been remarked, the fact that parhypate in all genera, including the diatonic, is indicated by the same alphabetic sign.

An obvious inconsistency will have occurred to the reader. *Ἐκβολή* is evaluated at five dieses. Similarly the highest interval of the soft diatonic of Aristoxenus is a tone and a quarter. I have been interpreting this as equivalent to the interval of the septimal third ( $\frac{7}{5}$ ), which occurs in Archytas' system as the product of a major tone and the small septimal semitone ( $\frac{8}{7} \times \frac{5}{4}$ ). But in the discussion of the chromatic and diatonic nuances this same semitone has been taken to be equal to Aristoxenus' third-tone. The distinction is small, whatever Aristoxenus meant by his quarter-tone, but still Aristoxenus makes it. In strict consistency, then,  $\frac{7}{5}$  should appear in Aristoxenus' system as  $1\frac{1}{5}$  tones. He should, for instance, have evaluated his soft diatonic:  $\frac{5}{4} \times \frac{3}{2} + 1\frac{1}{5}$ , which would then have corresponded exactly with Ptolemy's *σύντονον χρώμα*. To this may be replied that he would not countenance  $\frac{5}{12}$  in his scheme any more than  $\frac{7}{12}$  for the higher enharmonic pycnon. To the contention that he ought to have evaluated the *ἐκβολή* as  $1\frac{1}{5}$  I can only reply that he was determined that his enharmonic should have quarter-tones, that he knew the *ἐκβολή* was an enharmonic interval, that he hoped and believed it was represented by a tone plus his enharmonic diesis, and that he was convinced that the lowest interval of a tetrachord was never larger than the middle one.

If we can, then, equate Aristoxenus' soft diatonic with Ptolemy's *σύντονον χρώμα* with a high degree of probability, does Ptolemy's own soft diatonic,  $\frac{3}{2} \times \frac{10}{9} \times \frac{8}{7}$  ( $85 + 182 + 231$ ), find no reference in Aristoxenus? Not, clearly, in his system of nuances. But there is a difficult passage in pseudo-Plutarch *de mus.* (§§ 394-407) which I think provides a mention of it. The speaker (and his authority is almost certainly Aristoxenus), answering the objection that the enharmonic quarter-tone, which he is defending, cannot be obtained by means of the consonances, turns the tables on his opponents by remarking that this objection applies equally to the intervals also which consist of an odd number of dieses. Now, they themselves prefer to employ tetrachordal divisions in which the intervals are for the most part either odd or irrational; for they are continually lowering (*μαλάττονσι*) the *λιχανοί* and *παρανήται*. (The continuation raises a fresh problem of considerable difficulty which is not relevant here.) In what genus did this lowering of lichanos take place? Not in the enharmonic, for it is the raising of lichanos that he cavils at there. More likely it is in the diatonic. The *σύντονον* diatonic for all theorists except Ptolemy has as its upper interval a major tone. The popular tendency was to lower the lichanos and produce a 'soft' diatonic. This could be done in two ways, by substituting for the tonè ( $\frac{9}{8}$ ) either the septimal tone ( $\frac{7}{6}$ ) or the septimal third ( $\frac{7}{5}$ ). In the latter case there resulted what Ptolemy called a *σύντονον* chromatic and Aristoxenus



a 'soft' diatonic, of which the two upper intervals were three and five dieses, that is to say *περιττά*. In the former case there resulted Ptolemy's 'soft' diatonic. Now the highest interval of this is in Aristoxenian language  $1\frac{1}{2}$  tones, and this is an irrational interval (on any possible theory of that debatable term which is consistent with the evidence of the fragment on Rhythm).<sup>1</sup>

This article must have seemed to the reader a mass of hypotheses, some more plausible, some less. No one is more conscious than the writer of the number of loose ends that remain. This was inevitable in dealing with such an unscientific author as Aristoxenus. But it seemed to me worth while to attempt to extract musical sense from his simple arithmetic; and in some cases, notably his implied admission of the diatonic and chromatic of Archytas, the sense appears to be so good that it creates a certain presumption that his other dicta are not wholly nonsensical. The acceptance of Archytas' ratios means in effect the acceptance of the seventh harmonic as an important element in Greek music. What is more likely to provoke opposition is the use of the eleventh harmonic to explain the three-quarter-tone interval (the musical probabilities are hard to estimate); and some may prefer, despite the difficulties I have mentioned, to find in it the minor tone and so equate the *ἡμιόλιον χρώμα* of Aristoxenus with the chromatic of Eratosthenes, his soft diatonic with Ptolemy's soft diatonic. In any case the absence of this latter interval from the above interpretation of Aristoxenus' nuances is a serious matter. Rather, however, than find it in the three-quarter-tone, I would believe that it has been lost by attempting to ascribe too great accuracy to the author. The *τονιαῖον χρώμα* is a crucial case. Evaluated in cents it comes closer to the type with a major tone in the pycnon than to that with a minor tone. But it must be noted that twice Aristoxenus' leimma-semitone is almost exactly a minor tone. It is conceivable that the upper interval of this tetrachord may represent not only  $\frac{32}{27}$  but  $\frac{8}{5}$ ; that in the latter case its pycnon may, on the principle of common *παρυπάται*, have been divided into a major and a minor semitone (though on Aristoxenus' principles it would have been in the reverse order to Didymus). It is conceivable that he intended his *σύντονον* diatonic to represent either the ditonal diatonic or that with a major and a minor tone (probably in Didymus' rather than Ptolemy's order). He may in either case have disregarded the difference of a comma. But I have already stated why I find it hard to believe that the theorist who was agitated by the raising of the enharmonic lichanos by that interval (and how else can the *συντονωτέρα λιχανός* be interpreted?) disregarded that difference everywhere else. I could believe more easily that he deliberately banished the minor tone from his system along with the major semitone because he could not express them in simple enough fractions, because they were associated with the enharmonic lichanos he deplored, because they disarranged his theory of the loci of parhypate and lichanos. It may even be the case—and this hypothesis would save both the appearances and the reputation of Aristoxenus—that the minor tone was not in fact an interval in common use in this period of Greek music; that the harsh thirds of the ditonal scale, which were sweetened in the enharmonic by substituting the major third, were habitually modified in the diatonic by an alteration in the other direction to give the large septimal third ( $\frac{9}{8} = \frac{8}{7} \times \frac{9}{8}$ ).

However that may be, the chief service of Aristoxenus' account of the genera seems, on examination, to be the confirmation of Archytas.

R. P. WINNINGTON-INGRAM.

TRINITY COLLEGE, CAMBRIDGE.

<sup>1</sup> Aristoxenus' remarks upon the lowering of lichanos here and his polemic against its raising in the enharmonic combine to show the primary importance of this string in determining genus and nuance. As for the two lowest intervals, those of Ptolemy's tetrachord might be expressed

as  $1\frac{1}{2}$  and  $1\frac{1}{3}$  of a tone, and would thus both be irrational. It would be possible to fill up the tetrachord also with  $\frac{3}{2} \times \frac{8}{5}$ , that is to say  $\frac{1}{2} + 1$ , intervals more familiar to Aristoxenus. But this has no independent support.

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## VERGIL, PROBUS, AND PIETOLE, AGAIN.

My 'Further Considerations on the Site of Vergil's Farm'<sup>1</sup> have drawn from Professor Rand two more long but lively articles in which he seeks again to defend Pietole and to controvert the evidence of the manuscripts of Probus. The effect of his articles on the mind of any reader who has not both time and inclination to test Professor Rand's statements by comparing them with the passages in his own and in my writings, to say nothing of others to which he refers, is almost certain to be two-fold. First an impression that the whole question depends on highly technical points, of manuscript criticism and of the interpretation of puzzling scholia, on which no one but an expert can hope to form a judgement. And, secondly, the feeling that if so devoted a student of Vergil as Professor Rand, declaring himself quite content with the mediaeval tradition, can accept with a perfectly light heart interpretations of what Vergil wrote about his own farm which leave the reader in the end quite in the dark as to where it was and what it was like, then the non-specialist scholar may safely leave the matter in that obscurity, only thanking Professor Rand cordially for the charming gaiety with which he has handled and appears to have dismissed a troublesome enigma.

For truth's sake I am bound to recall the facts of the case, especially because new evidence on some of the points in which Professor Rand is most interested has just been offered by Professor Remigio Sabbadini, who is universally recognized as the first of all living authorities on the tradition of the text of Vergil. But I confess that my task is one which I would gladly have avoided, because, however much I pass over, the facts of the evidence cannot be re-stated without my showing in how many matters what I have said has been completely, though I know unconsciously, misrepresented by an old friend. He thinks in each case that I have misrepresented him; the reason for his thinking so is partly because more than once he has expressed quite different opinions in different places about the same point, and partly because of his habit of making rather large general statements the effect of which is to throw dust, in his own eyes and in the eyes of the reader, upon the particular points really at issue. If I attempted to deal with all the discrepancies, the controversy would sink into the 'he-said-that-I-said-that-he-said' type of dispute, of no importance to anyone but our two selves. In what follows I shall do my best to confine myself to points which bear directly upon the evidence for the site of Vergil's farm. For the rest I must be content to say that if any reader can find time to examine our respective statements, he will find that the misunderstanding and consequent misrepresentation were not on my side.

Let me begin with Professor Sabbadini's contribution to two parts of the question. One concerns the text of the *Life of Vergil* attributed to Probus, and the other concerns the actual character of the place Pietole as compared with what Vergil says in *Eclogue IX*. In an article entitled '*La Vita di Virgilio di Valerio Probo*'<sup>2</sup> Sabbadini examines the history of the text, and shows from two catalogues, one dating from 1461 and one of the tenth century, of the library of the famous monastery of Bobbio, that Pomponius Laetus had in his hands at Rome not earlier than 1461 and not later than 1470 two manuscripts from Bobbio—namely, (1) the great manuscript of Vergil which is now known as the Medicean and (2) the *Life and Commentary* attributed to Valerius Probus, both of which are named by Bussi,

<sup>1</sup> *C.Q.* XXV (1931), p. 65.

<sup>2</sup> *Historia*, Gennaio-Marzo, 1932.

the Bishop of Aleria, who was connected with the second Roman edition of Vergil in 1471. This edition contains the Life of Vergil which we are discussing, and this document Bussi mentions as having been received from Pomponius. In a course of lectures on Vergil in Rome, Pomponius quoted this Life and one of his pupils published certain notes of it in the cod. Ambrosiano R. 13 dated December 16, 1478.

What then is the conclusion which Sabbadini<sup>1</sup> draws from this history? He writes thus (I translate literally): 'The chief source of the text of Probus is the Codex Vatic. (lat. 2930), which is superior to all the other copies [of the Bobbio manuscript] and very greatly superior to the corrupt and lacunose copy which was used for the edition of Egnatius [of 1507]. This Vatican Codex was possessed and used by Pomponius but not written in his own hand.'

Sabbadini then gives the text of the Life in what he regards as its correct form, with critical notes showing readings of this codex, of the Roman edition of 1471, of the manuscript of Crinitus (cod. Monac. lat. 755 f. 4) written in 1496, and of the edition of Egnatius published at Venice in 1507. Among the comments which Sabbadini adds upon the text are these—'*Milia passuum* XXX. This is the genuine reading for which Egnatius substituted *millia passuum* III, either through some injury of the manuscript before him or by his own conjecture.' On this Sabbadini subsequently remarks: 'I believe Egnatius' reading was derived rather from a conjectural correction than from a blot in his manuscript, because at that period among humanists the Mantuan tradition was very widely known ("divulgatissima"), a tradition which set Vergil's native place at Pietole, close to the so-called *mons Virgilii* two or three miles from Mantua. This *mons* was visited just in the years during which Egnatius was preparing his edition by Giovanni Bremio, who left this description of it: "Locum appellant incolae Montem Virgilii, qui cum ad Mintii ripam sit a quo iactu lapidis valido lacerto iacti distet sitque iter sursum versus faciendum et ascensu superandus fluminis alveus, cognovi propterea montem esse ab illis dictum, cum tamen nullus ibi mons habeatur."'

This quotation comes from the work of Dal Zotto, a book which Professor Rand also quotes, on *Vicus Andicus* (Mantua, 1930, p. 132). Sabbadini then continues: 'Of course Bremio could not find a *mons* in a place where there was no *mons* and never was. ("Naturalmente non poteva trovare monte dove monte non era e non fu mai.") Nevertheless we ought to have been able to find ("ci dovette essere") the *mons Virgilii* (Verg. *Buc.* IX, 7 *colles*). Where it was we do not know, just as we do not know when and by what road the legend transported it to the banks of the Mincio at Pietole.'

I quote the last negative utterance from Sabbadini, because it shows how completely independent of anything that I have written his account of the matter is. Now Professor Sabbadini lives at Pisa and was for a long time professor at Milan, and has of course visited Pietole himself. His confirmation of Bremio's statement, that the so-called *mons Virgilii* was simply a slight rise in the bank of the river and distant a stone's throw from the water, exactly agrees with my own recollection of the spot where Professor Nardi most kindly took me when, in April, 1930, I visited Pietole for the third or fourth time; and I note further that among all Professor Rand's (quite admirable) pictures from Pietole, of which he gives thirteen, there is not one which shows the least rise of ground.

And yet, in his latest article,<sup>2</sup> Professor Rand actually writes thus: 'The elevation known as the *mons Virgilii* answers well enough to his [Vergil's] *colles*.' This kind of statement almost defies discussion. If Professor's Rand's devotion to

<sup>1</sup> It would be superfluous to reproduce the whole of Sabbadini's brief and cogent account, but I may generally refer to his famous book *Le scoperte dei codici Latini e Greci*, Florence, 1905,

which, with the full references he gives in his present article, enables the student to verify his argument step by step.

<sup>2</sup> *C.Q.* XXVI (1932), p. 70.

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Pietole can persuade him to write in this way about a small swelling of twenty or thirty feet within a stone's throw of the river, does it not imply that no facts really count for anything in his mind unless they accord with his chosen creed? The same question applies to his remark on the following page: 'If my memory serves me correctly, there is no view of distant mountains from Carpenedolo.' Of course there is not if Professor Rand took his stand immediately to the south of one of the houses or towers of the village; but if he looked up the northward street or from any point of the ridge on the front of which the village stands (and on the top of which at the W. end lay, as I believe, Vergil's farm) he would have had the same fine view of the Brescian hills as my photographs showed from Calvisano.

How can I persuade Professor Rand of the inadequacy of his interpretation of Vergil, if interpretation it can be called to point to a spot within a stone's throw of the river and identify it with the 'hills' between which and the river, according to Professor Rand's account, stretched all the land which had been taken from Mantua and which Octavian promised in vain to restore? Let me at least remind him that he has already planted these *colles* in two different spots, each of which, in its turn, he said was fifteen miles from Mantua. On pp. 115 and 116 of his *Quest of Vergil's Birthplace* he puts them among the northern hills by Valeggio; but on p. 150 of the same book he puts them not north of Mantua, but west, on the boundary of Cremona!

On the whole manuscript question I am content to leave the issue between the ingenious structure of conjectural possibilities which Professor Rand puts forward—though he has considerably changed it since it first appeared—and the new and definite facts put forward by Professor Sabbadini. Yet I need not even leave it so, because Professor Rand himself, after many pages of strained argumentation in an endeavour to show that Egnatius' edition of 1507 is a better authority for what the Bobbio codex contained than the manuscripts of 1460 or 1470 and the edition of 1471, actually arrives at this conclusion (p. 7): 'By that time, however, some forty years had elapsed [since the making in 1460 of the earlier copy which was used by Pomponius Laetus] and the condition of the ancient book had deteriorated.'

In the face of this admission I cannot understand Professor Rand's contention that the four witnesses to the text other than and older than Egnatius (Sabbadini cites three of them) should only be counted as equivalent to one, and that one Egnatius. Even if we granted Professor Rand's repeated but to me doubtful assertion<sup>1</sup> that these four sources were all derived from a single copy of the Bobbio codex made in 1460, they would still be four witnesses to what that codex contained at that date as reported by the copy, and this Professor Rand (*l.c.*) admits: 'Their evidence might help to determine more nicely just what the type is.' Whereas Egnatius is only one witness to what another copy of it contained some thirty-six years at least after the earliest copy was made: and, as Sabbadini has both asserted and shown, a not very trustworthy witness either. And Professor Rand himself persists in confirming this estimate, though much against his will. For despite the pages which he devotes to whitewashing this humanist worthy, his own theory is that Egnatius depended on a copy of the Bobbio codex, whereas Egnatius himself writes and Professor Rand himself quotes twice, as a passage of great importance, the statement of Egnatius: 'I have followed the ancient text as I noted it from the

<sup>1</sup> Professor Rand thinks I have treated him hardly by saying that he confined himself to the part of the manuscripts which contained the *Life* when he had 'pointed out "a number of other certain instances"' from the parts containing the *Commentary*. Let me quote the note in his book (*Quest of V.'s Birthplace*, p. 168) to which

he refers; the reader will see that it contains a bare assertion without quoting a scrap of evidence: 'A slight examination of the text of the *Commentary* following the *Life* (in Thilo and Hagen's edition) reveals at once a number of other certain instances; these need a careful study.' I have no doubt they do.



ancient codex once found by Georgius Merula at Bobbio' ('*secuti sumus vetustatem illam quemadmodum ex vetustissimo codice manu scripto Bobii quondam a Georgio Merula inuento adnotauimus*'). Of this Professor Rand writes that 'his statement is not a lie if someone else made the copy, whether the manuscript was at Bobbio or elsewhere'; and in order to soften Egnatius' statement Professor Rand represents the phrase *adnotauimus ex* by the words 'depends on.' I submit that Egnatius cannot be acquitted of an untruth in view of the facts now ascertained and admitted by Professor Rand.

Before leaving the manuscript side of the question I must be allowed to express my surprise that Professor Rand should spend more than a page in discussing my estimate of the age of the Vatican and Paris manuscripts as compared with that of Crinitus. In view of his own admission it is now irrelevant to the question at issue, except perhaps that it might serve to discredit his opponent's view on questions of manuscript criticism. Therefore, I suppose I must point out that Professor Rand himself immediately afterwards (p. 5) dates them about 1460, that is, thirty-six years earlier than Crinitus (who, as we have seen, dates his manuscript at 1496). This is only ten years later than my own explicitly tentative estimate.

The danger into which Professor Rand has fallen through his habit of general statements broader than the point at issue appears very plainly in his strange remark (p. 12) 'It is the only certain rule that I have found (from at least thirty years' experience with MSS.), namely, that a scribe is capable of almost anything.' Professor Rand's own doubt as to this 'certain rule' is shown by his putting in the word 'almost.' No one can deny such a statement. Anything may happen in any part of life. But to apply it as Professor Rand suggests here, and as hundreds of other brilliant people have done before, to mean that it is lawful to propose an emendation without showing any reasonable ground<sup>1</sup> for believing that the reading proposed could and would probably have given rise to the corruption found in the manuscript has done and still does more harm to the first business of the scholar, the ascertainment of what his author wrote, than any other weakness of the human mind. Such at least is the conviction formed in my mind by thirty years of experience not merely 'with MSS.' but of constructing a text from the study of them.

Another of the methods by which Professor Rand endeavours to discredit the *Life* by Probus is to lump it together with the Commentary on the *Bucolics* and *Georgics* that follows it, which is a totally different affair, as Nettleship long ago pointed out (see his Essay in Conington's *Virgil*, Ed. vi (1881), pp. lxx ff.). By combining these two different things in one statement, Professor Rand is able to say without direct mis-statement that 'the main interest of the compiler is mythological.' The reader would certainly not gather from this the fact which Sabbadini again emphasizes, that the *Life* attributed to Probus is the only one that altogether excludes the element of fable, and the only one whose chronology, so far as it goes, is both precise and correct. Indeed, Professor Rand goes further in his misrepresentation of the document. He actually declares that it contains blunders 'hard to attribute to a scholar like Probus.' He gives only one example, and it is the only point that there is even an excuse for so regarding. 'Probus states' (says Professor Rand) 'that the evictions at Mantua . . . occurred after the battle of Actium.' This statement cannot be maintained of the *Life* as we have it. The word *Actium* nowhere appears in what Probus wrote, and the clause which Professor Rand<sup>2</sup> chooses to refer to Actium occurs in the sentence about Mutina; and this has a lacuna for which Professor Rand expressly commends my restoration (c.2. *l.c.*, p. 4). With the same freedom of dog-

<sup>1</sup> Professor Rand's ingenious guesses as to how III might have come to be read as XXX leave me quite cold. Let him produce an example.

<sup>2</sup> I have never seen this curious view taken by anyone else. The man who abridged Probus wrote *Augustus* where the context shows that he meant *Octavianus*.

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matic statement Professor Rand declares that Probus' placing of Andes at thirty miles from Mantua 'flatly contradicts that in Donatus.' Donatus says *non procul*. Surely it is amazing to find such a scholar as Professor Rand treating the word *procul* as if it were a geometrical term. Does not Professor Rand know that *procul* may denote any degree of separation, from a few inches, as when Silenus' garland tumbled off his head, to the distance of an exile from his fatherland, say, of Tomi from Rome? It is all a question of where you are when you are writing. By a writer in Rome or even in Milan, any *vicus* of Mantua would naturally be said to be *non procul*, 'not far' from that town.

I am bound to notice further the serious misquotation from myself with which Professor Rand begins his second article (p. 65). He puts between inverted commas as quoted from me words that I did not write at all. Professor Rand wrote on p. 96 of his *Quest*: 'In view of the plentiful amount of Magian inscriptions and their wide dispersion in North Italian towns, the presence of one of them at Casalpoglio is not particularly striking evidence.' I answered this by pointing out that the Magius of Casalpoglio had a wife whose cognomen was Sabina, which implied a connection with Vergil's own people much closer than that merely implied by membership of the Magian gens. This point Professor Rand completely escapes by means of his (of course unconscious) misquotation.

This brings us to Professor Rand's discussion of the two inscriptions. He persists in talking about 70 B.C. as though anyone had asserted that date for either of them, saying that I 'apparently' do not mean to stretch their date further back than 50 B.C. The adverb contains a really false suggestion. I need not repeat what I have carefully stated in my original paper, and implied again in my 'Further Considerations,' about the precise evidence which can be extracted from the two inscriptions and the places in which they were turned out of the ground, in the light of Probus' statement. Again on p. 65 he declares that he had distinguished between public and private inscriptions in discussing the date of the two which concerned Vergil's family. He mentioned the distinction, no doubt; but all the inscriptions which he reproduced on p. 101 of his *Quest* are public inscriptions, and on p. 100 he expressly compared two of these ('Look at the two inscriptions') with the two under discussion. I cannot help wondering how far Professor Rand has allowed his masterly knowledge of Latin writing on vellum to colour his judgement of writing on stone, which is a very different thing. It is impossible to answer his surprising assertion (*Quest*, p. 98) that the character of the capitals of the *Vergilia*-insc. 'verges upon that which is called Rustic,' when, even in the wide freedom of space which he enjoyed in that charmingly illustrated story of his wanderings, he does not mention one single point of resemblance. That he is not very familiar with actual Latin epitaphs is suggested by the strange misinterpretation (*Quest*, p. 98) of V.F. (which of course means *uius fecit*) as if it were V.S. (*uotum soluit*); even a rich farmer did not make a vow to himself and his wife (*sibi et uxori*).<sup>2</sup>

Let us come back to Vergil's own words in *Eclogue IX*, *qua se subducere*

<sup>1</sup> See e.g. Verg. *E.* VI. 16; Ovid, *Trist.* 5. 4. 13.

<sup>2</sup> Among points where Professor Rand is still unconscious of the serious way in which he has misrepresented either the facts or my account of them, I note without discussing (1) his inadequate withdrawal of his mis-statement as to the distance of Carpenedolo from Mantua, which he repeats in his *Magical Art of Vergil*, p. 103 (published in 1931), though he had withdrawn it in the notes attached to his *Quest*, p. 169 (published in 1930); and (2) of his continued misconception (p. 69) of what I said about the scene of the

*Eclogues* with odd numbers; on (3) the *pericam-porraxerat*-passage he still totally refuses to notice the difficulties which I pointed out in the interpretation which he dogmatically assumes; and (4) though he generously acknowledges his mistake about Bianor (p. 71), he does not realize that his original mis-statement had changed into a strong argument for his view what is in fact a strong argument against it. A multitude of smaller points *sciens praeterire*. Most of them are as irrelevant now as the *q* of Crinitus, on which I am only too glad to be 'put wise.'

*colles incipiunt . . . usque ad aquam.* By means of playing about with a totally imaginary object which he calls 'the firing line' Professor Rand escapes my challenge as to how he interprets Vergil's statement. I asked whether he thought Vergil's farm was or was not included in the stretch of land that Vergil names. Professor Rand now seems, though not very definitely (p. 70), to believe that it was included; but he nowhere meets my point that if the hills were in the north, which we have seen was his first view, the farm, if it was at Pietole, must be excluded by the words *usque ad aquam*, since Pietole is at the south end of the south-east lagoon. Since, however, he now, as we have seen, places the *colles* close by the river Mincio, the whole breadth of the land concerned in Vergil's words so interpreted is reduced to 'a good stone's throw'. The truth is that Professor Rand attaches no importance at all to what Vergil says, but every importance to defending by hook or by crook the mediaeval tradition; and so long as he allows himself to be blinded by this suprarational affection, so long he will be capable of a looseness of thought and statement which those who know the absolute integrity and generosity of his intentions, and the splendid work which he has accomplished in other fields, must do their best to forget as quickly as possible.

R. S. CONWAY.

ST. ALBANS, September, 1932.

P.S.—I see that Professor Rand will expect an answer on one more point. He demurs (*i.e.*, p. 70, footnote) to my criticizing the looseness of Professor Nardi's statements (about the changes in the water-courses round Mantua made by Pitentino) by saying that there is 'plenty of documentation in the revision of Nardi's book, translated by Mrs. Rand (*Youth of Vergil*, 1930).' The reader who refers to the passage in the translation will only discover what the word 'documentation' means to Professor Rand and his 'no mean authority, Professor Nardi,' namely, some three pages of assertions which, beginning with 'not improbably,' 'we may suppose,' and other modest phrases, quickly pass into categorical statements, for which the only evidence given is a long list of works ranging from 1811 onwards (on the hydrographic and hydraulic history of the district), without the least indication as to where in all the books cited the statements made by Professor Nardi are to be found or justified. οὕτως ἀταλαίπωρος ἡ ζήτησις τῆς ἀληθείας. And, after all, the whole talk about Pitentino is totally irrelevant, since there is no reason to doubt what Professor Nardi himself admits—that the lagoon extended from the north-west to the south-east of Mantua in Vergil's time (p. 129) as it does now. This, combined with the facts that Pietole lies at the south end of this lagoon, and that the *colles*, as Professor Rand has (once) admitted, must be in the north, reduces to nonsense, as I have pointed out, Vergil's phrase *usque ad aquam*, if Vergil's farm is placed at Pietole.

In the one case in these pages where Professor Nardi does give a precise reference, if he had quoted it in full he would have found that it completely fails to prove what he thinks it does; Strabo (5. 1. 6-7) in enumerating the chief towns north of the river Po, refers to half a dozen (Mediolanum, Verona, Brixia, Mantua, Regium and Comum) as being 'far above the marshes,' αὐται μὲν οὖν πολὺ ὑπὲρ τῶν ἐλῶν κ.τ.λ.; but he continues immediately πλήσιον δὲ τὸ Πατάονιον, which shows that by 'the marshes' he means those at the mouth of the Po; and this is abundantly clear in all the rest of the section, as he says that the tide comes up to Patavium 'through the marshes,' and mentions Ravenna and Altinum as 'lying in the marshes.' Professor Nardi's other reference to Strabo, about the marshes which Hannibal crossed on the south of the Po, is quite correct and quite irrelevant.

## NOTE ON 'VIRGIL'S BIRTHPLACE REVISITED.'

(C.Q. APRIL, 1932, PP. 66 FF.)

Je désire rectifier quelques assertions du professeur E. K. Rand au sujet de mon livre *Les Masques et les Visages dans les Bucoliques de Virgile*.

1. Ce n'est pas de Mantoue mais de Tusculum que Meliboeus a été évincé selon moi.

2. Je n'ai donné aucun rôle à Agrippa dans les Bucoliques et je n'ai jamais écrit qu'elles étaient une 'chronique scandaleuse.'

3. Mais Ovide exilé a bel et bien considéré les Bucoliques comme une œuvre légère et hardie justifiant ses propres ouvrages, car il a écrit (*Tristia* II. 537-538):

Phyllidis hic idem teneraeque Amaryllidis ignes  
bucolicis iuuenis luserat ante modis.

4. Et il a, comme moi, reconnu que les châtaignes caractérisent Amaryllis en écrivant (*Ars Am.* II. 267-268):

Adferat aut uuas aut quas Amaryllis amabat,  
at nunc castaneas non amat illa nuces.

Plutôt que d'insister sur ces erreurs ou fausses représentations de détail, il me semble utile de porter le débat sur le point principal. J'ai essayé de montrer que dans les *Bucoliques* chaque personnage porte un pseudonyme unique. Alors, si Menalcas désigne Virgile (*Buc.* V. 85-87), Meliboeus ne peut être Virgile, car Menalcas dit (*Buc.* III.)

dic mihi, Damoeta, cuium pecus? an Meliboei?

et Tityrus ne peut être Virgile, car Menalcas dit (*Buc.* V.)

Incipe: pascentis seruabit Tityrus haedos.

Par suite, la première bucolique n'a rien à voir avec l'éviction de Virgile. Les seuls vers qui puissent nous renseigner sur la propriété perdue par le poète sont les vers 7-10 de la IX<sup>e</sup> bucolique, et, comme ils sont prononcés par *Lycidas* dans le voisinage du tombeau de Bianor, c'est à dire juste à mi-chemin entre Brescia et Vérone (voir ma communication à l'Académie des Inscriptions et Belles-Lettres du 14 Nov. 1930 qui sera reprise dans un article sur la Topographie des Bucoliques dans la *Revue Archéologique*), ils situent la propriété de Virgile dans la contrée de collines qui s'étend entre le lac de Garde et Mantoue, ce qui me semble en faveur de la théorie de Mr. R. Conway.

LÉON HERRMANN.

UNIVERSITÉ DE BRUXELLES.

## SUMMARIES OF PERIODICALS.

### LITERATURE AND GENERAL.

**Classical Philology.** XXVI. 4. October, 1931.

F. E. Robbins, *Arithmetic in Philo Judaeus*: P. shows familiarity with an arithmetic of the Pythagorean type as later expounded by Nicomachus; his lost *περὶ ἀριθμῶν* to which he refers was probably not an arithmetic but an arithmology on Pythagorean allegorical lines. J. H. McCarthy, *Octavianus Puer*: collects references to Octavian's youth from Cicero and Dio and suggests (following Dio and Suet.) that the senate's contempt for his youth was the cause of his change of attitude. C. R. Morey, *The Vatican Terence*: concludes (from the copy Basilicanus H 19) that C was written at Corbie before the migration to Korwey—i.e. not later than the first quarter of the ninth century—and suggests that the scribe and painter of its hyparchetype ( $\gamma^2$ ) may both have been Greeks.  $\gamma^2$  passed from Corbie to Reims (where P was copied) and perhaps thence (along with P) to Fleury with Archbishop Ebbo: F's hyparchetype  $\gamma^1$  was written not earlier than the second half of the fifth century, which confirms Craig's view that the Calliopian recension belongs to the fifth century. W. Peterson, *The Subjunctive in Cum-Clauses in Early Latin*: rejects Hale's theory of the derivation of temporal *cum*-clauses with subjunctive from 'descriptive' *qui*-clauses: from analysis of early usage concludes that the order of development of the subjunctive constructions was (1) with *cum* adversative-concessive (already in Liv. And.), (2) with *cum* causal (? first in Ter., with *praesertim*), (3) with *cum* temporal (? first in Cato), and that (3) was a mechanical extension of (2) [and perhaps (1)], due originally to the failure of the hearer to catch the speaker's thought, and probably never had any real meaning. J. J. Savage, *Was the Commentary on Virgil by Aelius Donatus extant in the Ninth Century?*: confirms the view of Thilo and Rand from an unnoticed marginal note in the Bernensis of Servius (*ad Georg.* ii. 4), which seems to imply that, when the MS. was written before the end of the ninth century, a text of the complete commentary of Aelius Donatus was at Liège, where Sedulius may have used it. W. F. J. Knight, *Epilegomena to 'The Wooden Horse'*: supplements his view (*C.P.* xxv. 358 sqq.) that the horse was a magical device to break the ring-magic of the wall. F. R. B. Godolphin, *The Nemesis of Cratinus*: concludes that the play was written by the elder Cratinus (not the younger who, *pace* Capps, cannot have been his son), that it alluded to Pericles (as Plut. *Per.* 3 says), that it was produced in 432/1 and that the Schol. on Ar. *Av.* 521 placed it later through confusing the archons of 432/1 and 404/3, both named Pythodorus. G. B. A. Fletcher, *Two Notes on Plutarch De Mal.* Herod.: 3. 855D *πρὸς τοὺς ἐπαίνοισι*, retains the text, comparing Ar. *Eq.* 193; 43. 874B prefers B's *μικρολογίαν* = 'disparagement,' comparing Isocr. xv. 2. 310B and citing other parallels between Plut. and Isocr. J. A. O. Larsen suggests that Narbonensis and Gallia Cisalpina were under the same governor from 67 to 65 B.C.; Dio 36. 37. 2 and Cic. *Att.* i. 3. 2 suggest that C. Calpurnius Piso became governor of Narb. in 67, while Cic. *Att.* i. 1. 2 and Sall. *Cat.* 49. 2 imply that he was governing Cis. shortly after; he is more likely to have held the provinces concurrently than in immediate succession. P. Shorey explains Arist. *Pol.* 1324a 2: what 'some will dispute' is not the identification of the virtue of the individual and that of the state but the identification of virtue and happiness.



## XXVII. 1. January, 1932.

B. L. Ullman, *Classical Authors in Certain Mediaeval Florilegia* (V.): examines the evidence of the *florilegia* (and Vincent of Beauvais' citations from them) for *Culex* and *Aetna*, Calpurnius and Nemesianus, and (less fully) for other Latin authors: he adds some notes on the origin of the *florilegia* and their relation to the mediaeval school-syllabus, their importance for the history of mediaeval culture, and the method employed by Vincent (a new edition of whose works is urgently needed). Kenneth Scott, *Tiberius' Refusal of the Title 'Augustus'*: T.'s practice of confining official use of the title to letters to foreign powers is connected with his religious policy: acceptance of it in Rome, where its associations were still fresh, would have amounted to consecration, which he refused to countenance. C. E. van Sickle, *Conservative and Philosophical Influences in the Reign of Diocletian*: criticizes the usual view of D. as a ruthless innovator and finds evidence in the codes that he regarded himself as a follower of Roman traditions: suggests from this and from his treatment of the succession that he was influenced by philosophical advisers of Stoic sympathies and was trying to adapt to his times the Stoic theory of government. Gertrude Gerther, *Pompeian Magistri*: discusses the *magistri Aug. Merc. Mai.* of C.I.L. X. 884-923, suggests (from parallels at Tibur, Nola and Delos) that their original functions were connected with the *compita* and that the cult of Augustus was added to an earlier cult of Mercury and Maia (after Aug.'s reform of the worship of the *compita*) and finally supplanted it, and finds here further evidence for a cult of Mercury-Augustus. J. A. O. Larsen, *Alexander at the Oracle of Ammon*: criticizes interpretation of Callisthenes fr. 14 (Strabo 17. 1. 43) by Wilcken and Pasquali and holds (1) that Diodorus and Curtius are describing an entirely different ceremony from that described by Call. (2) that Call.'s words (if they are his and not distorted by Strabo) are not necessarily incompatible with the statement in other sources that the attribution of divinity to Alex. took the form of a greeting. R. S. Rogers, *Two Criminal Cases heard before Drusus*: examines consequences of (1) the summary execution of Clutorius Priscus, (2) the conviction of Annia Rufilla for malicious use of the image of Augustus as asylum, which probably led to the *senatus consultum* on the subject (Dio. 47. 10. 38) and to the regulation of rights of asylum in the Greek East. G. M. A. Grube, *The Composition of the World-Soul in Plato*, *Timaeus* 35A-B: retains αὐτῆς and accepts Proclus' interpretation (Diehl II. 156) against modern commentators, who identify 'same' and 'other' with noetic and phenomenal being: the *Ψυχὴ* is composed of three ingredients, each a blend—mixed οὐσία, mixed ἕτερον and mixed αὐτό—each intermediate between the divisible and indivisible. K. Scott on Suet. *Vesp.* 12 proposes to supply *cupide* (or the like) in place of *statim*, which is accepted by most edd. though inconsistent with the facts. D. M. Rowbathan draws attention to a quotation from Diodorus in the unpublished work *De laboribus Herculis* of Coluccio Salutati: the translation was made for him by Leonardo Bruni at least forty-three years before Poggio's version and at least seventeen years before Aurispa records his bringing a MS. of Diodorus to Italy. P. Shorey on Plato *Rep.* 598E notes that ἀνάγκη γὰρ . . . ποιεῖν expresses the opinion which P. is (for his immediate purpose) rejecting.

## Mnemosyne. LIX. 3.

W. E. J. Kuiper, *Menandri incerta fabula*. Among the fragments of Menander which have come to us from Cairo there is a passage containing some seventy verses from a comedy the name of which is unknown. Van Leeuwen and others have attempted to deduce the plot of the scene. K. offers some suggestions, and argues that the play was the *Canephorus* mentioned by Suidas. W. Vollgraff, *Inscriptiones Traiectenses*, discusses the inscriptions found in 1929 in the city of Utrecht. Among other noteworthy features, there occur the names of certain Celtic deities hitherto unknown and the local name Albiobola. V. supposes that Traiectus (known from

c. 300 A.D.) was the name of a place on the right bank of the Rhine, Albiobola on the left. The inscriptions seem to belong to the early years of the fourth century. B. A. van Groningen on Pindar, *Pyth.* XI., discusses the problem involved in v. 13. ἐν τῷ (Κιρρίας ἀγωνί) Θρασύδαϊος ἔμνασεν ἐστίαν τριτόν ἐπὶ στέφανον πατρίαν βαλὼν, as to the other two victories referred to. G. thinks that Πυθονίκῳ (v. 43) is a proper name—the name given to Thasydaeus' father in memory of the Pythian victory won by T.'s grandfather, who subsequently won an Olympian victory. The obscurity in the reference to these victories is due to the fact that Pythionicus, who had commissioned the poet to write the ode, though son and father of a victor, had won no victory himself, a fact which the poet would naturally not wish to bring into prominence. S. Tromp de Ruiter, *De vocis quae est Φιλάνθρωπία significatione et usu*, traces the word from its earliest appearance down to its use by the Christian fathers. The adjective φιλάνθρωπος is first found in Aesch. *Prom. Vinct.* 8 and 28; the noun does not appear till Plato, *Euthyphro* 3D, and Xenophon, *Memorabilia* IV. 3, 7. In meaning it corresponds closely to the Latin *humanitas* except that the latter is not used of the relation of God to man. *Ad Titulum Laconicum*. G. V. discusses the meaning of ΝΑΡΓΩ ΧΑΙΡΕΙΝ (*B.S.A.* XXIX., p. 56). He thinks this = Νάργῳ χαίρειν, and for the name cf. Νηκλῆς, Νήνομος, Ναμένης. As the last of these is certainly formed from νη- and ἀμενής, so he thinks Νάργος is from νη- and ἀργός. H. Wagenvoort, *De Sticho Plautina*, argues (after P. Lejay) that the structural peculiarities of the *Stichus* can be explained by assuming that the poet's object was to depict incidents illustrating 'The Warriors' Return Home.' C. Brakman contributes critical notes on Avienus. J. D. Meerwaldt proposes an emendation on Longinus, *De Subl.*, ch. 4. After quoting from Xen. *Rep. Lac.* (3. 5) αἰδημονεστέροις δ' ἂν ἡγήσαιο καὶ αὐτῶν τῶν ἐν τοῖς ὀφθαλμοῖς παρθένων, Longinus proceeds ὁ μέντοι Τίμαιος ὡς φωρίον τινὸς ἐφαπτόμενος οἷδ' ἐτοῦτο τῷ Ξενοφῶντι τὸ ψυχρὸν κατέλιπεν (Timaeus had written of some disgraceful act, ὁ τίς ἂν ἐποίησεν ἐν ὀφθαλμοῖς κόρας μὴ πόρνας ἔχων;). M. instead of φωρίον would write φωτίον, an (assumed) diminution of φῶς with the sense *inveniuncula quaedam luminosa*. There are short notes on *Perv. Ven.* 9, 32; Theocr. XIV. 31 and on the accounts of the treatment of the Rhegines by Dionysius of Syracuse contained respectively in Diod. XIV. 111, 4 and [Aristot.] *Oecon.*, 1349b 17.

**Neue Jahrbücher für Wissenschaft und Jugendbildung.** VII. 8. 1931. VIII. 1-4. 1932.

(VII. 8) H. Berve, *Sulla*. Sulla was the last representative of the old instinctive Roman statesmanship scarcely touched by Hellenistic thought. His belief in the patronage of Aphrodite was deep and genuine. M. Dibelius, *Glaube und Mystik bei Paulus*. Deals largely with Albert Schweitzer's *Die Mystik des Apostels Paulus*, and attempts to define the mystical element in Paul's thought. (VIII. 1) J. Stenzel, *Hegels Auffassung der griechischen Philosophie*. After analyzing the sources of our knowledge of Hegel's views on Greek philosophy, S. discusses the permanent value of his judgments, and finds it considerable, especially for Plato and Aristotle. (2) F. Altheim, *Almus sol*. An analysis of the *carmen saeculare*, especially in connection with Apollo and Diana, and an assertion of the novelty and sincerity of the religious feeling behind the *ludi*. (3) E. Fraenkel, *Tacitus*. A penetrating analysis of the development of Tacitus's historical attitude from the *Dialogus* to the later books of the *Annals*. (4) L. Curtius, *Goethe und die Antike*. An account of the main external facts of Goethe's contacts with ancient literature and art, and a study of the inner significance of the antique upon his personality and writing. K. von Fritz, *Die neue Interpretationsmethode in der klassischen Philologie*. The essence of the 'new method' is the desire to approach works of ancient literature from the standpoint of their own time and circumstances, concentrating especially on their unlikeness to modern forms.

Tycho von Wilamowitz's work on Sophocles and several later books are considered from this angle.

**Philological Quarterly (Iowa).** XI. 1. January, 1932.

A. P. Dorjahn, *The Athenian Senate and the Oligarchy of 404/3 B.C.* Discusses the Senate who sat just before and under the Thirty. They were more independent than is generally supposed, and were not oligarchic in character, number and function.

XI. 2. 1932.

G. McCracken, *John Wilkes, Humanist*. An account of Wilke's classical studies. R. C. Flickinger, review of Parts 4 and 5 of the new edition of *Liddell and Scott's Greek Lexicon*.

**Rheinisches Museum für Philologie.** N.F. LXXX. 2. 1931.

A. H. Krappe, *Helena*. Of the two myths of the abduction of Helen, the Homeric is derived, the Theseus-myth the original, its early form having been of a common type in which a vegetation-goddess is carried off to Hades and subsequently freed. Theseus is a substitute for the original Aphidnos (= Hades), and the scene is localized at Aphidna. The type goes back to an Indo-European original in which Jack in the Green carries off the May Queen. W. Büchner, *Die Niptra in der Odyssee*. Examines the context of Od. 19, 346-8, explaining away the difficulties pointed out in the scholia. A. Klotz, *Über einen Schreibgebrauch Catos*. After a full discussion of Quintilian 1, 7, 20 (read *dicae et faciae* for Halms *dicem et faciem*) and 9, 4, 37 sqq. (read *dicae faciaeque* in 39) with pertinent parallel passages and glosses, he agrees with Lindsay and C. H. Morris that this is not a case of a peculiar 1st pers. fut. indic. in -em to be found in Cato, but of an attempt to mark in writing the different pronunciation of final -m. Klotz then shows from Velius Longus (G.L. 7, 80, 17) that the symbol used was the first half only of the letter M (P) which before Quintilian had been misread as E. R. Philippson, *Sind die Dialoge Augustinus historische?* Answers in the affirmative in opposition to A. Gudemann, whose arguments he meets seriatim. Ph. Finger, *Die drei kosmologische Systeme im zweiten Buch von Ciceros Schrift über das Wesen der Götter*. Analyzes sources of *de nat. de.* II. and elucidates three (in opposition to Reinhardt and Heinemann)—those of Posidonius, Antiochus, and Panaetius. Detailed analysis devoted to the teaching of these systems on the cosmos, stars, divine governance of the world, and divine providence over earthly things. Main distinctions are (a) Posidonios—Aether is the supreme source of all spiritual, vital and motive forces, and the world is periodically resolved into it: (b) Panaetius—no element supreme, and the world is eternal: (c) Antiochus—divine principle different from all the elements: a spiritual Demiurge who has constructed the everlasting world from chaotic matter and is resident in the heavens.

**Rivista di Filologia.** N.S. IX. (1931) 4.

R. Mondolfo, *Intorno al contenuto dell' antica teogonia orfica*. This article is part of the editorial *addenda* written for a forthcoming Italian version of Zeller's *Philosophie der Griechen*. The author re-examines the subject at length in the light of evidence discovered, or first seen to be relevant, since Zeller's time. A. Rostagni, *I primordii dell' evoluzione poetica e spirituale di Virgilio*. VII. *Occasione e autenticità della Ciris*. While he was at Naples, though he wrote little poetry, Vergil did not abandon it altogether: cf. *Catalepton* V. R. argues strongly that the *Ciris* is by Vergil himself. It was conceived about the same time as the *Culex*—in 48 B.C.—but it was touched up at intervals until V. finally sent it to Messalla in 45. Messalla is the consul of 31 B.C. G. Lombardo, *Alessandro Filelleno*. The philhellenism of Alexander I belongs to the



c. 300 A.D.) was the name of a place on the right bank of the Rhine, Albiobola on the left. The inscriptions seem to belong to the early years of the fourth century. B. A. van Groningen on Pindar, *Pyth.* XI., discusses the problem involved in v. 13. ἐν τῷ (Κυρῶς ἀγῶνι) Θρασύδαος ἐμύσεν ἐστίαν τρίτον ἐπὶ στέφανον πατρίαν βαλὼν, as to the other two victories referred to. G. thinks that Πυθονίκῳ (v. 43) is a proper name—the name given to Thasydaeus' father in memory of the Pythian victory won by T.'s grandfather, who subsequently won an Olympian victory. The obscurity in the reference to these victories is due to the fact that Pythionicus, who had commissioned the poet to write the ode, though son and father of a victor, had won no victory himself, a fact which the poet would naturally not wish to bring into prominence. S. Tromp de Ruiter, *De vocis quae est Φιλανθρωπία significatione et usu*, traces the word from its earliest appearance down to its use by the Christian fathers. The adjective φιλάνθρωπος is first found in Aesch. *Prom. Vinc.* 8 and 28; the noun does not appear till Plato, *Euthyphro* 3D, and Xenophon, *Memorabilia* IV. 3, 7. In meaning it corresponds closely to the Latin *humanitas* except that the latter is not used of the relation of God to man. *Ad Titulum Laconicum*. G. V. discusses the meaning of ΝΑΡΓΩ ΧΑΙΡΕΙΝ (*B.S.A.* XXIX., p. 56). He thinks this = Νάργω χαίρειν, and for the name cf. Νηκλῆς, Νήνομος, Ναμένης. As the last of these is certainly formed from νη- and ἀμενής, so he thinks Νάργος is from νη- and ἀργός. H. Wagenvoort, *De Sticho Plautina*, argues (after P. Lejay) that the structural peculiarities of the *Stichus* can be explained by assuming that the poet's object was to depict incidents illustrating 'The Warriors' Return Home.' C. Brakman contributes critical notes on Avienus. J. D. Meerwaldt proposes an emendation on Longinus, *De Subl.*, ch. 4. After quoting from Xen. *Rep. Lac.* (3. 5) αἰδημονεστέρους δ' ἂν ἡγήσαιο καὶ αὐτῶν τῶν ἐν τοῖς ὀφθαλμοῖς παρθένων, Longinus proceeds ὁ μὲντοι Τίμαιος ὡς φωρίον τινὸς ἐφαπτόμενος οἷδὲ τοῦτο τῷ Ξενοφῶντι τὸ ψυχρὸν κατέλιπεν (Timaeus had written of some disgraceful act, ὁ τίς ἂν ἐποίησεν ἐν ὀφθαλμοῖς κόρας μὴ πόρνας ἔχων;). M. instead of φωρίον would write φωτίον, an (assumed) diminution of φῶς with the sense *inventumcula quaedam luminosa*. There are short notes on *Perv. Ven.* 9, 32; Theocr. XIV. 31 and on the accounts of the treatment of the Rhegines by Dionysius of Syracuse contained respectively in Diod. XIV. 111, 4 and [Aristot.] *Oecon.*, 1349b 17.

**Neue Jahrbücher für Wissenschaft und Jugendbildung.** VII. 8. 1931. VIII. 1-4. 1932.

(VII. 8) H. Berve, *Sulla*. Sulla was the last representative of the old instinctive Roman statesmanship scarcely touched by Hellenistic thought. His belief in the patronage of Aphrodite was deep and genuine. M. Dibelius, *Glaube und Mystik bei Paulus*. Deals largely with Albert Schweitzer's *Die Mystik des Apostels Paulus*, and attempts to define the mystical element in Paul's thought. (VIII. 1) J. Stenzel, *Hegels Auffassung der griechischen Philosophie*. After analyzing the sources of our knowledge of Hegel's views on Greek philosophy, S. discusses the permanent value of his judgments, and finds it considerable, especially for Plato and Aristotle. (2) F. Altheim, *Almus sol.* An analysis of the *carmen saeculare*, especially in connection with Apollo and Diana, and an assertion of the novelty and sincerity of the religious feeling behind the *ludi*. (3) E. Fraenkel, *Tacitus*. A penetrating analysis of the development of Tacitus's historical attitude from the *Dialogus* to the later books of the *Annals*. (4) L. Curtius, *Goethe und die Antike*. An account of the main external facts of Goethe's contacts with ancient literature and art, and a study of the inner significance of the antique upon his personality and writing. K. von Fritz, *Die neue Interpretationsmethode in der klassischen Philologie*. The essence of the 'new method' is the desire to approach works of ancient literature from the standpoint of their own time and circumstances, concentrating especially on their unlikeness to modern forms.



Tycho von Wilamowitz's work on Sophocles and several later books are considered from this angle.

**Philological Quarterly (Iowa).** XI. 1. January, 1932.

A. P. Dorjahn, *The Athenian Senate and the Oligarchy of 404/3 B.C.* Discusses the Senate who sat just before and under the Thirty. They were more independent than is generally supposed, and were not oligarchic in character, number and function.

XI. 2. 1932.

G. McCracken, *John Wilkes, Humanist*. An account of Wilke's classical studies. R. C. Flickinger, review of Parts 4 and 5 of the new edition of *Liddell and Scott's Greek Lexicon*.

**Rheinisches Museum für Philologie.** N.F. LXXX. 2. 1931.

A. H. Krappe, *Helena*. Of the two myths of the abduction of Helen, the Homeric is derived, the Theseus-myth the original, its early form having been of a common type in which a vegetation-goddess is carried off to Hades and subsequently freed. Theseus is a substitute for the original Aphidnos (= Hades), and the scene is localized at Aphidna. The type goes back to an Indo-European original in which Jack in the Green carries off the May Queen. W. Büchner, *Die Niptra in der Odyssee*. Examines the context of Od. 19, 346-8, explaining away the difficulties pointed out in the scholia. A. Klotz, *Über einen Schreibgebrauch Catos*. After a full discussion of Quintilian 1, 7, 20 (read *dicae et faciae* for Halms *dicem et faciem*) and 9, 4, 37 sqq. (read *dicae faciaeque* in 39) with pertinent parallel passages and glosses, he agrees with Lindsay and C. H. Morris that this is not a case of a peculiar 1st pers. fut. indic. in *-em* to be found in Cato, but of an attempt to mark in writing the different pronunciation of final *-m*. Klotz then shows from Velius Longus (G.L. 7, 80, 17) that the symbol used was the first half only of the letter M (𐌚) which before Quintilian had been misread as E. R. Philippson, *Sind die Dialoge Augustinus historische?* Answers in the affirmative in opposition to A. Gudemann, whose arguments he meets seriatim. Ph. Finger, *Die drei kosmologische Systeme im zweiten Buch von Ciceros Schrift über das Wesen der Götter*. Analyzes sources of *de nat. de.* II. and elucidates three (in opposition to Reinhardt and Heinemann)—those of Posidonius, Antiochus, and Panaetius. Detailed analysis devoted to the teaching of these systems on the cosmos, stars, divine governance of the world, and divine providence over earthly things. Main distinctions are (a) Posidonios—Aether is the supreme source of all spiritual, vital and motive forces, and the world is periodically resolved into it: (b) Panaetius—no element supreme, and the world is eternal: (c) Antiochus—divine principle different from all the elements: a spiritual Demiurge who has constructed the everlasting world from chaotic matter and is resident in the heavens.

**Rivista di Filologia.** N.S. IX. (1931) 4.

R. Mondolfo, *Intorno al contenuto dell' antica teogonia orfica*. This article is part of the editorial *addenda* written for a forthcoming Italian version of Zeller's *Philosophie der Griechen*. The author re-examines the subject at length in the light of evidence discovered, or first seen to be relevant, since Zeller's time. A. Rostagni, *I primordii dell' evoluzione poetica e spirituale di Virgilio*. VII. *Occasione e autenticità della Ciris*. While he was at Naples, though he wrote little poetry, Vergil did not abandon it altogether: cf. *Catalepton* V. R. argues strongly that the *Ciris* is by Vergil himself. It was conceived about the same time as the *Culex*—in 48 B.C.—but it was touched up at intervals until V. finally sent it to Messalla in 45. Messalla is the consul of 31 B.C. G. Lombardo, *Alessandro Filhelleno*. The philhellenism of Alexander I belongs to the

period after the Persian invasion. When it came, it may have been such as to commend itself to Sparta; but it did not involve any sympathy with Athenian ambitions in the North. A. Momigliano, *Intorno al Contro Apione*. The writer first discusses the logical nexus between the two books. He then suggests that Josephus used the *Hypothetica* of Philo, and that his knowledge of Manetho is almost wholly derived at second hand from tendentious literature based on M. C. Gallavotti, *Sopra un opuscolo perduto di Dione Crisostomo*. In Suidas s.v. Δίων the author proposes to correct Ἐγκώμιον Ἡρακλείους καὶ Πλάτωνος to κατὰ Πλ. Miscellanea. I. Q. Cataudella, *Sul. 'Pap. Heidel.'* 314 KATA ALEXPOKEPΔEIAΣ. The author illustrates the sense from Gregory Nazianzen, *Iamb.* XXII, and offers new restorations of lines 16 ff. II. M. Guarducci, *Ancora sull' iscrizione sepolcrale di Aptera*. The text is one published in *Rivista*, 1929, pp. 378 ff. The editress now prints some readings suggested by von Wilamowitz and von Hiller. A revised translation is added. *Recensioni. Note bibliografiche. Cronache e commenti. Pubblicazioni ricevute.*

### LANGUAGE.

**Zeitschrift für vergleichende Sprachforschung.** 60. Band. 1./2. Heft. 1932.

This number contains an important article by F. Hartmann on the dialect of Herodotus as it appears in the MSS. He shows that the dialect differs from that of the Ionic inscriptions in ways that go to show that false Ionicisms were introduced into it by the Alexandrines. The grammarians had fixed ideas of what Homeric language ought to be, and they made the text of Ionic writers conform to these. He traces the steps by which certain forms came to be in the text of Herodotus. A note follows by W. Schulze on the affinities of Cret. *καπραινος*. H. Grewolds contributes a long article on the relationship of the Gothic compounds in the text of the remains of the Gothic Bible to those in the Greek text, dealing first with verbal and secondly with nominal compounds.

W. Krogman examines at length the origin and etymology of the German Jul (Eng. Yule). He also contributes an etymological note on *Old Norwegian* ællugu, øllykti. There is an article by F. Specht on *Germanisch* Harigasti, and by E. Schwyzler on *Got. let und griech. έα*. W. Schulze as usual contributes further etymological notes. Finally E. Hofmann concludes his interesting contribution on *Kultur und Sprachgeist in den Monatsnamen*, dealing with the Lithuanian and Lettish month names and comparing their meaning with that of the Greek, Latin and other names.

Adcoo  
gove  
Aeneid  
Aesch  
Alexan  
Lett  
Allen  
antiqua  
Antony  
Aristo  
Aristo  
52 ff  
Aristo  
195  
Artemi  
August

Baldry  
Socr  
Broad  
metr

Caesar  
14 ff  
Campb  
30, 4  
Campb  
elder  
Conwa  
again  
Cornfo  
266A  
Ctesib

diminu  
distich

Encho

Festus

Gow (A  
150 ff  
Greek  
195 ff

Herrma  
revisi  
Hesych  
Higham  
103 ff  
Hill (H

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